THURSDAY, JUNE 6, 1889.

REPORT OF THE ROYAL COMMISSION ON A UNIVERSITY FOR LONDON.

THIS Report is a disappointment. The spectacle of three eminent lawyers taking an eminently legal view of a question, and three teachers an educational view, is instructive and amusing, but it is not business. Passing over, for the present, the question how far its conclusions are discredited beforehand, by the dissent, on the material point at issue, of all the Commissioners who have had experience of teaching, we shall consider the principal Report from our own standpoint; which is that of a complete impartiality as between the University of London, the petitioning Colleges, and the other institutions and interests concerned, and of an earnest desire to see established in London a real University for teaching and research—that is to say, one of which the function is the dissemination and advancement of knowledge, while the examinations are relegated to their proper place, as accessories to study, not fetters on the teaching.

At first sight the impression is favourable. "The general case for a teaching University is," in the Commissioners' opinion, "made out." The limitation of its area to London, so far as concerns its teaching functions, is strongly insisted on, and reiterated in several paragraphs. The rejection of a separate University for Medicine is good in itself, and is of good omen, when the position of the two leading Commissioners is considered, for a future association with the University of the only possible London School of Law, that of the Inns of Court. The Commissioners have adopted some excellent ideas as to the constitution of Faculties, consisting exclusively of actual teachers, and as to the formation from the Faculties of Boards of Studies. They further "think it desirable to give a definite value to the training and teaching which those students will obtain who go through the prescribed courses of constituent colleges and teaching institutions connected with the University," and they formulate certain proposals for their exemption from specified examinations, which at least serve to show that the recognition thus given to the value of systematic study under competent guidance is not, in their minds, a purely nominal one. There cannot be a doubt that, if their recommendations were adopted, the administration of the University of London would be improved, and the influence of its examinations upon study, in London at all events, greatly modified for the better. We do not see why, subject to some further consideration for the case of students elsewhere than the Commissioners have cared to give, the reforms suggested in the Report should not now be carried out, as it proposes, by the independent action of the University of London, with hearty support and approval from the University Colleges, and without in the smallest degree impairing their case for the simultaneous establishment as a separate institution of a real teaching University for London.

But this, of course, is not the intention of the Commissioners. On the contrary, they recommend, in so many terms, that if this reform should take place in the University of London, "the prayer of University College and King's College be not granted." This obliges us to examine whether the reform in question will insure to us that further benefit to be expected from the establishment of a teaching University which lies apart from any possible good results to follow upon the mere perfecting of the machinery of examinations. Upon this head it is our deliberate judgment that they will fall short of what is needed. It is not the somewhat stinted amount of representation upon the Senate which is offered to University College and King's College; not the representation on a more liberal scale of teachers in the University Colleges and schools, arranged in Faculties for the purpose, upon the same body; not even the appointment by these Faculties of so-called Boards of Studies, limited to the function of advising the examining body; nor yet all these concessions taken together, which will transform a general examining body into a teaching University for London, as we understand the term. Far less will such a University be constituted by "confederating" or "co-ordinating," for examining purposes only, all the "various societies and institutions in London which profess to give advanced teaching," such as, for example, "the Birkbeck Institution, the City of London College, and the Working Men's College." In the Charter of the Victoria University it is expressly provided that the affiliated Colleges shall be, for teaching purposes, efficient. This necessary safeguard has been overlooked by the Commissioners. There remains the recommendation that "the University should have power to teach by professors and lecturers of its own, attached or unattached to particular Colleges or institutions, and to receive endowments for that purpose." Assuming the necessary endowment to be forthcoming, the bearing of this proposal depends on its application. If the University Colleges are willing to allow of the "attachment" to them of University professors—that is to say, to allow the University to appoint the leading members of their teaching staff—and if further they permit the University "Boards of Studies" to arrange their prospectuses, then, indeed, the result will be that the Colleges, for teaching purposes, will become merged in the University; and we shall have, at the expense of the sacrifice of their individuality, and not without considerable violence to their traditions, a strong and homogeneous teaching University in London. Otherwise, one of two things will happen: the separate University staff may be a mere peripatetic staff of lecturers, doing what is known as "University extension" work; or else it will constitute a third University College, with a privileged position, competing with others for their students. In neither of these cases shall we have reaped the characteristic benefit of a teaching University, which is, shortly, the organization of teaching power.

Thus, on the most careful examination we have been able to give to the proposals of the Report, we are brought to the conclusion that only on one condition—a condition very unlikely to be realized—will they result in giving us a real teaching University for London. In default, we shall have a general examining University as before, but one largely under the control of London teachers. The change will entail a violence to the traditions of the University, and perhaps a disparagement of its Imperial position. The Commissioners, indeed, say: "For other parts

of the Kingdom, as for the Colonies, it is sufficient that there should be access, as heretofore, to examinations and to degrees." This dictum, a curiously inaccurate one, if we consider the recent foundation of the Victoria University, not to speak of Colonial and Indian teaching Universities, will hardly be accepted by the country Colleges which still look, and must for some time continue to look, to the University of London for degrees. They will claim representation on its governing body, if not also on its Faculties and Boards of Studies; and if its functions are restricted to the control of examinations, we do not see on what principle the claim can be refused. The refusal to admit institutions at a distance is justifiable if the first business of the University is to teach; but hardly plausible if its function is only to examine.

Nor is this all. In order to obviate "the risk of practical injustice being done to candidates for degrees from country Colleges or from no Colleges at all," the Commissioners propose to establish what they call a "balance in the government of the University and in determining the course of the examinations and the choice of examiners," between the associated institutions and "independent elements." This they attempt by assigning a majority in the governing body to Crown nominees and the representatives of Convocation. This provision will in no way help to keep the University in touch with the teaching of the country Colleges; but it will undoubtedly entail the risk, which has been overlooked by the Commissioners, of producing a want of flexibility in the administration of the examinations, considered as accessories to study in the London Colleges. The University will thus start on its teaching career with a clog about its movements. Differences of opinion are sure to arise among its administrators, as often as an alteration of its programme is proposed in the interests of the London teaching; and these will be differences which no wisdom or moderation will entirely obviate, because they will be due to a fundamental difference in the point of view. We strongly apprehend that an institution so framed will be found to contain within itself the seeds of failure. In any case it will hardly obtain that large measure of confidence from the Colleges which would lead them to intrust it with any power over their teaching.

The initial fallacy of the whole Report may be traced to a little word in the twelfth page of it, paragraph 15. The Commissioners say: "Whatever difficulties there may be in the way of establishment of such form of connection as may be desirable between a teaching University for London and the different bodies and agencies now engaged in kindred work on an independent footing, we think it probable that these difficulties may be more easily overcome if *the ground* were occupied by one University only, and not by two." The ground proposed to be covered is not one field of work, but two. There is the work of affording by means of a general examination a test of attainment for students in institutions of as yet imperfect efficiency, and for private students. There is also the work of organizing the teaching, with its examinations closely following and dependent on the teaching, in the efficient Colleges of London. The two differ essentially, not merely in area, but in purpose. The attempt to devise an instrument competent to regulate both at once has failed, as it was bound to fail. Whatever line is now taken by the University of London, we trust that the promoters of the movement, who have succeeded hitherto so far beyond expectation, will stand fast by their principles, and not forfeit, by a too great eagerness for immediate results, the success which is certain ultimately to crown their efforts.

TASMANIAN GEOLOGY.

Systematic Account of the Geology of Tasmania. By Robert M. Johnston, F.L.S. Pp. 408, with Geological Map and Sections, and 57 Plates of Fossils. (Hobart: Published by the authority of the Government, 1888.)

NEARLY forty-five years have elapsed since Count Strzelecki and Prof. J. Beete Jukes, working independently, made known to geologists the main features of the important island of Van Diemen's Land. In the interval between the publication of their researches and the present day, numerous papers treating on questions of local geology have been published by Mr. Charles Gould—who for a time was engaged in making a geological survey of the colony—and by many amateur geological investigators. No complete description of the geology of the whole colony has as yet appeared, however; and we therefore heartily welcome the large and comprehensive volume now lying before us, as supplying a long-felt and pressing want.

The author of this work, Mr. Robert M. Johnston is the Government Statistician and Registrar-General of Tasmania; and during the last sixteen years he has devoted much time and labour to the study of the geology and natural history of the colony in which he resides, and has published numerous papers dealing with questions of stratigraphical geology and palæontology, as well as of botany and zoology. In the year 1884, Mr. Johnston was requested by the Tasmanian Government to write a general treatise on the geology of the island; and the present work has been prepared during the leisure hours of a busy Government official.

The Island of Tasmania has an area of a little more than 26,000 square miles, or between four and five times that of Yorkshire. Over large parts of the island there is a covering of almost impenetrable scrub; while the rivers are large enough to make traverses of the country by no means an easy task; and the rainfall is heavy. The interesting details of the methods of exploration, given in the introductory chapter of this work, illustrate the nature of the difficulties which have had to be overcome in making the researches upon which the work is based. We cannot but admire the energy and zeal which have been exhibited in carrying out the numerous and valuable observations that have made the present work possible.

The excellent sketch-map of the geology of Tasmania, drawn on a scale of 15 miles to the inch, gives a very good idea of the general distribution of the several rock-masses. The oldest formations appear on the west and on the north-east of the island, and consist of crystalline schists, apparently belonging to the Archæan periods, associated with clayslates, quartzites, sandstones, and limestones of Cambrian, Ordovician, and Silurian age, with some small and doubtful representatives of the Devonian. The palæontological evidence concerning the

age of the different Lower Palæozoic rocks appears to be of a fairly satisfactory character.

Lying in the district between the two areas of older rocks, we have, in the central parts of the island, a tract of great extent, which is occupied by the important coal-bearing strata. These strata have, however, been greatly invaded by igneous extrusions, and are, over a considerable area, covered up by Tertiary deposits. While the lower series of these coal-bearing strata contain the remains of plants, like those of the Carboniferous strata of Europe and the United States, the higher Coal-measures yield many plants having Mesozoic affinities.

Mr. Johnston, like all who have had to deal with the geology of countries in the East and in the southern hemisphere, has been compelled to confront a very serious difficulty—that of making his nomenclature and classification fit in with the scheme that has been adopted in the countries which happen to have been the first systematically studied by geologists. His biological training and knowledge have here, however, stood him in good stead; and there are few contributions to this difficult question more worthy of attentive consideration than the chapter of this work which deals with nomenclature and classification, and the suggestions offered by the author on the subject of the "distribution of genera in time, from independent or widely-separated geographical centres."

Mr. Johnston divides his Tertiary strata into the two groups called by him Palæogene (including perhaps the Eocene, Oligocene, and Miocene of European geologists) and Neogene (corresponding with our Pliocene). It is perhaps unfortunate that in the latter case a name is employed which has also been used by the geologists of Eastern Europe with a somewhat different signification.

Coming down to post-Tertiary times, the author gives an excellent account of the caverns and native shell-mounds, containing the rude flint-implements of the aboriginal inhabitants. Portraits are given of the last surviving man and woman of the Tasmanian race (King Billy and Truganini), the former of whom died in 1869 and the latter as recently as 1876. Drawings of the rude instruments made of chert which were used by this interesting race of human beings, and details concerning the mode in which the natives employed the different kinds of weapons, will prove of great service to those engaged in studying the remains left by various ancient races in Europe and America.

Full justice is done to the different kinds of igneous rocks, so far as they have yet been studied; to the various economical products; and especially to the useful ores, of the island. Interesting details are given concerning the mode of occurrence of the "Tasmanite," or "white-coal," which attracted so much attention a few years ago, and first led to the investigation of many similar "spore-coals" in Europe and America. The character of the deposits from which stream-tin and gold have been obtained is also described, and their importance is indicated by accurate statistics: the value of the tin obtained in Tasmania is now shown to be between £300,000 and 400,000 per annum. Nor are more purely scientific and theoretical questions neglected. An interesting discussion of the probable distribution of land and water in the Australasian region before and during the Tertiary period is illustrated by sketch-maps; and here, too, the author's biological knowledge has aided him greatly in dealing with a very complex and difficult problem.

But quite independently of the scientific value of the work, which as we have seen is certainly very great, we think the Government and people of Tasmania are to be congratulated upon the character of this remarkable and handsome volume. It aims at being above all things of practical use, and its great object is to direct the attention of the colonists to questions of pressing interest and importance, as well as to secure their aid and co-operation in solving the important problems presented by the geology of the country.

The bibliography of Tasmanian geology has, with the assistance of Mr. Robert Etheridge, Jun., been very amply dealt with. Chapters containing a key to the determination of rocks, and instructions for the blowpipe examination of minerals, together with an excellent glossary of geological terms, which might seem out of place in a memoir on European geology, will make this work of service to many colonists who have not had the advantage of a scientific training or access to libraries. The numerous plates, too, if not so highly finished in some instances as we are accustomed to in works of the kind, serve their purpose admirably; and the plan of giving side by side with the imperfect fossils found in the colony a number of well-marked types from the other Australian colonies, and even from Europe, can scarcely fail to prove of the greatest service to many a traveller or resident in the country, whose only work of reference may be this volume.

In the execution of his task, which has evidently been a labour of love, the author has received much assistance from the geologists in other Australian colonies and in New Zealand, and this he warmly acknowledges. No less valuable has been the co-operation of many of his fellow-colonists, who have aided him by drawing plates, in making special inquiries, and in many other ways.

When invited to undertake the work, the author was requested to prepare a volume which should be "specially suited for the guidance of local students, mining prospectors, and others." We can heartily congratulate Mr. Johnston, and the Government which have so liberally paid for the publishing of the book, upon having not only completely accomplished their primary object, but of having at the same time issued a work which is of the highest scientific value. It is not often that the wants of the general public and of the scientific specialist have been so admirably met; or that a book has been produced, which is at the same time accurate and thorough in its treatment of technical questions, while it is not wanting in the more elementary details required by those who have not had the advantages of a scientific training.

JOHN W. JUDD.

CACTUS CULTURE FOR AMATEURS.

Cactus Culture for Amateurs. By W. Watson. Profusely Illustrated. (London: L. Upcott Gill, 1889.)

QUAINTNESS of form, extraordinary brilliancy of colour in the flowers, facility of cultivation, all supply reasons why these plants, independently of the

scientific interest attaching to them, should be extensively cultivated. At one time succulents were fashionable. They were grown, and grown well too, in the most unpromising localities—on the leads of London houses, in back yards, in cottage windows. They may still be found in the latter position, where the conditions often seem to suit them better than in more specialized habitations, probably because the plants get full exposure to light, and a dry atmosphere, while they are at the same time relatively impervious to dust. Barring occasional survivals, succulents have, however, "gone out." A collector recently dead did his best to galvanize the public taste. Regardless of expense, he got together a superb collection. With unwonted generosity he lent large assortments to public institutions, where they might be seen of many. With even greater liberality he gave away great numbers of plants to schools and other institutions, as well as to private individuals. He hoped by so doing to revive the public taste for this class of plants. Vain hope! They are no more abundant now than they have been for the last quarter of a century; the nurseries have mostly discarded them as unprofitable lumber; the great collections are broken up. Even the assemblage to which we have just alluded has been recently sold under the hammer, and, despite its excellence, realized scarcely more than enough to pay for the hammer. Under these circumstances it was with no little astonishment that we saw a series of articles in the *Bazaar* some time since, and it is with even greater surprise and pleasure that we now welcome their republication. The publisher, it seems, noting the general absence of cactuses in English gardens, came to the conclusion that the reason for their exclusion was to be found in the absence of adequate knowledge as to their cultivation and management. He thereupon commissioned Mr. Watson, Assistant Curator of the Royal Gardens at Kew, to write a series of chapters on the subject. Brave publisher! may he be amply compensated for his chivalrous efforts! At any rate, he pursued a wise instinct when he secured the services of Mr. Watson. While private collections have, as we have said, almost disappeared, these plants have always been very well represented at Kew. The succulent house indeed is, and always has been, one of the most striking features of that grand establishment. While of exceptional interest to the naturalist, the cactus house appeals to the attention of the general public more forcibly, if not always more pleasurably, than any other department of the Gardens. Ten years' experience in the care and cultivation of these plants at Kew is alleged by the author as his justification in publishing the present book. But, in truth, the book furnishes its own vindication. It is written for gardeners, not for botanists, but it will be acceptable to both, and we shall not be greatly surprised if the book is more appreciated by the naturalist than by the gardener. Fashion rules in gardening as in other things, and Fashion says now we will have cut flowers and roses, and we will have orchids; by and by, perhaps, it will demand cactuses, although they do not furnish "cut flowers," and this demand will, we trust, be furthered by the book before us.

The naturalist is, if not wholly, still largely, beyond the influence of the caprices of Fashion. For him the limited geographical distribution and the extraordinary forms of these plants, their surprising adaptations to the conditions under which they grow, their remarkable metamorphoses, the absence or extremely reduced condition of their leaves, the co-relative thickness of stem and expansion of surface, offer a never-ending source of interest and investigation. The wonderful provision against undue evaporation, the protection against the ill effects of radiation, the amount of green surface exposed to the sun even in the practical absence of leaves, the defence against thirsty marauders afforded by the spines, the singular nature and arrangement of those spines—all these points, to say nothing of the, in many cases, incomparably gorgeous flowers, give these plants claims on the attention of philosophic naturalists beyond those offered by most others. Further, with occasional exceptions they are easily grown, demand relatively little space, attention, or expense, and are therefore specially fitted for the naturalist of moderate means.

Lastly, we may commend them to the attention of those botanists who have the management of botanic gardens. Those who have visited the smaller University Gardens in Germany or France know what depressing establishments they generally are, and how little they seem to contribute to the advancement of science; but if the managers of each garden were to take up the cultivation, one of one genus of plants, one of another, as means and opportunities permitted, they might do excellent service to botany at little cost, and in these specialties achieve results not possible of attainment in larger and more exacting establishments. The Bromeliads were taken up in this manner in the Liége Botanic Garden by Prof. Morren, who unfortunately did not live to complete his monograph. We allude to the subject here because no group of plants affords better opportunities for a thorough and comparative study of the inter-relations of structure, development, and life-history generally than does the order Cactaceæ. Whilst fashion fades and palls, the interest of scientific investigation is not only continuous but progressive. The cactuses are replete with problems of deep import to naturalists, and we trust that Mr. Watson's book may be the means of bringing about the solution of some of them.

OUR BOOK SHELF.

Lehrbuch der Vergleichenden Anatomie zum Gebrauche bei vergleichend anatomischen und zoologischen Vorlesungen. Von Dr. Arnold Lang, Inhaber der Ritter-Professur für Phylogenie an der Universität Jena. Erste Abtheilung. (Jena: Gustav Fischer, 1888.)

THIS "Lehrbuch" is the ninth edition of E. Oscar Schmidt's well-known "Handbuch." It has been thoroughly revised—indeed, in parts rewritten. As might be expected from the Professor of Phylogeny at Jena, the subject is treated from a quite modern standpoint. We have first the systematic arrangement of the tribes, orders, and classes of each group or sub-kingdom, with the characteristics of each. This is succeeded by some general observations on the group, then the general morphology is described, next we have the details of the various systems; the illustrations to the descriptive part of each chapter being selected with great care and judgment, and being in most cases refreshingly new. This first part of the volume commences with the Protozoa and ends with the Vermes. With the immense advance of zoological

knowledge it may be regarded as impossible that any one person can have an equal knowledge of all the groups into which the animal kingdom is now divided; and while at once acknowledging the care which has been shown in the compilation before us, one has only to study the chapters on the Plathelminthes and Vermes to find how the author's special and great knowledge of these groups has made this the most valuable portion of the present part. The Protozoa are divided into three classes—the Monera, Sarcodina, and Flagellata; among these last, such genera as Pandorina, Stephanosphæra, and Volvox are included without a hint being given that many regard them as plant forms. The Cœlenterata are divided into the Gastræadæ, Porifera, and Cnidaria, and the former class is made to include not only the Orthonectidæ and Dicyemidæ, but also the Physemaridæ. In the quoted literature on this group no reference is made to Prof. Ray Lankester's very impartial paper on a species of Haliphysema. These facts are referred to, not as criticisms on this valuable addition to an already large list of introductions to a study of the comparative anatomy of the animal kingdom, but rather as in their way indicating the standpoint from which this one has been written. The printing is excellent, and the style of the work is worthy of the house of Gustav Fischer, of Jena.

A Manual of Practical Solid Geometry. By William Gordon Ross, Major R.E. (London: Cassell and Co., Limited, 1888.)

THIS book follows in the main the lines of geometrical drawing as studied at the Royal Military Academy, Woolwich. It will be found to be a useful help to those who desire to have the power of producing accurate and workmanlike drawings. Orthographic projection of points, lines, and planes, system of vertical indices, and projection of curves and solids, are first dealt with, and are followed by simple cases of regular solids and solids of revolution, illustrated by drawings in elevation and plan. A series of solid geometry problems are next worked out on the index system, figures being drawn in the more difficult cases. Problems in connection with irregular surfaces, and relating to the defilade of works of fortification, are worked out, and also illustrated by drawings. The appendix contains a collection of examples of different kinds, and various hints and suggestions useful for draughtsmen.

Key to Lock's Elementary Trigonometry. By Henry Carr, B.A. (London: Macmillan and Co., 1889.)

THE examples given in this book are fully and clearly worked out, and in the elementary examples the author has added considerable detail to enable those reading the subject for the first time, and those who are studying it without the help of a teacher, to obtain a clear insight into the working of them. Great care seems to have been taken to insure accuracy, and from beginning to end a teacher would find it hard to add much in the way of supplementary explanation.

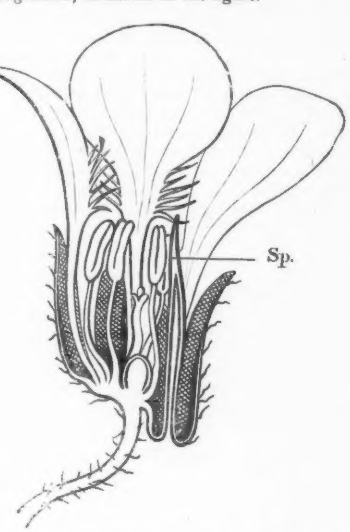
LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Abnormality in Tropæolum.

IN the early summer of last year my attention was directed to a case of abnormality in flowers of Tropæolum, which I think is sufficiently interesting and rare to be worthy of record.

I observed three distinct plants of several years' growth (in a conservatory) to be producing flowers in considerable numbers which were peculiar in having the spur either completely or partially *invaginated*, as shown in the figure.



In examples where the invagination is complete, the intruded spur occupies the exact position of the stamens, and it is a fact of some significance that it is not uncommonly double.

Flowers of the normal form were also developed, and I noticed that as the summer advanced the proportion of these increased until eventually the plants produced only flowers of the ordinary kind. They are again flowering this season, and are repeating their behaviour of last year in every detail.

I am not aware of any similar case having been recorded either in Maxwell Masters's "Vegetable Teratology" or elsewhere. The facts brought to light by the examination of numerous examples seem to me to suggest a new interpretation of the nature of the "spur" in this flower, which I purpose discussing at a later period. In the meantime I shall be very glad to hear of any similar instances either in this or any other "spurred" flowers. ALFRED DENNY.

Firth College, Sheffield, May 27.

The Structure and Distribution of Coral Reefs.

IN reply to Mr. Guppy's letter permit me to state that (owing to Captain Wharton's kindness) I had before me, when writing, the Report on the Survey of the Tizard and Macclesfield Banks. That reef-building corals occasionally grow at depths considerably greater than 25 fathoms was already known (see "Coral Reefs," second edition, p. 115, note), and Commander Moore's investigations did not appear to me to do more than confirm this. Mr. Guppy, I think, must have read his copy of the Report rather hastily, or he would hardly have failed to quote the following "suggestive remark" which occurs on p. 16:—"This fact [a living astræan at 45 fathoms] proves that the fine sand of the lagoon is not necessarily fatal to the solid reef-building astræan, and helps to explain how individual coral heads appear in the deep waters of these atolls, but it cannot be doubted that their growth is very limited. . . Coral growth is most luxuriant between 2 and 12 fathoms." T. G. BONNEY.

Atmospheric Electricity.

NOT once only, but on several occasions, I have been alarmed by the fizzing of my ice-axe in the Alps.

Twice in one neighbourhood—the Riffelberg—I have been in company with several tourists who have (as I myself) been considerably frightened.

It must be remembered that the Riffelberg and the Gorner Grat contain so much iron as to affect the compass observations of surveyors. How often may this be a determining cause?

Alpine Club, May 28. MARSHALL HALL.

Unusually Large Hail.

SOME very large hailstones fell here about 3.30 p.m. on Sunday, June 2, during a short but sharp thunder-storm. Most of them were ellipsoidal in outline; some were mammillated, and some were evidently compound, formed of several hailstones partially fused together. Ten picked up at random as being fairly large ones measured from $\frac{7}{8}$ to $1\frac{3}{4}$ inches ($\frac{7}{8}$, $\frac{7}{8}$, 1, $1\frac{1}{8}$, $1\frac{1}{8}$, $1\frac{1}{8}$, $1\frac{1}{4}$, $1\frac{1}{2}$, $1\frac{3}{4}$, $1\frac{3}{4}$ inches) in greatest diameter. Many of them were formed of four, five, or six concentric layers, which were alternately clear and snow white. Some of these hailstones lying on the grass took more than an hour and a half to melt away (temp. 65° F.)

Mr. I. C. Thompson has just drawn my attention to the residue which they leave when melted on a clean glass slide. This, when examined under a high power of the microscope, is found to contain, along with inorganic particles, a number of minute plant spores. W. A. HERDMAN.

University College, Liverpool, June 2.

The Muybridge Photographs.

ALLOW me to state, in order to save correspondence due to the omission of a publisher's name in connection with the Muybridge photographs (NATURE, May 23, p. 78), that they may be seen and ordered of Mr. Muybridge, at 38 Craven Street, Strand, London. E. RAY LANKESTER.

University College, London.

THE VICES OF OUR SCIENTIFIC EDUCATION.[1]

THE subject which I desire to bring to the notice of the Association to-day must necessarily remind you of the attack which was recently made on our competitive examination system in the *Nineteenth Century*.

In some respects the writers of the article, or articles, in the *Nineteenth Century*, appear to me to have right on their side when they object to the existing state of our competitive system.

They begin by complaining against the dangerous mental pressure, and the resulting physical mischief, which accompany the working of nearly all parts of our present educational system. This complaint seems to me to be just.

The fact is, in my opinion, that nearly all our examinations are much too difficult—too much beyond the mental and physical abilities of the examined.

Let me take, as a supreme instance, the examination for the Mathematical Tripos at Cambridge. Ever since I have known anything of this examination, I have wondered how it can be possible that young men, three years after leaving school, can successfully grapple with problems, a great number of which are of transcendent difficulty, in such an immense range of mathematical and physical subjects as this Tripos contains. Indeed, when we are admitted behind the scenes by reading the solutions of these problems by those who have set them, our wonder is increased; for we find frequently that the discussion of a single problem occupies four, five, or six (and sometimes more) pages of small print. Such problems have a very great value for the student who has plenty of time to consider them in the solitude of his study; but I should think that the attempt to attain the amount of knowledge and adroitness necessary to deal with them on the spur of the moment in the Senate House must often produce mental and physical injury. Are we really to believe that a young man of twenty-one or twenty-three has made himself master of nearly everything given to the scientific world by Newton, Laplace, Gauss, Jacobi, Helmholtz, Cayley, Thomson, and Clerk Maxwell?

The desire to place before a student a standard which (I suppose I am right in assuming) he can never reach is, of course, quite defensible; and it is one which appears in every educational competition. But it is not in all cases carried out with a regard for rightness of method. For, this desire to be always in advance of the student leads some examining bodies to hurry him through a large number of subjects in a short time. It is, I think, a marked characteristic of our very modern method that we require half a dozen branches of mathematics and physics to be got through in a time which, say, twenty years ago, would have been devoted to the study of two or three. Is there, for example, nearly so much time now devoted to the study of pure Geometry as there was then? Is Trigonometry so thoroughly and leisurely studied now in the schools?

With the rage which now exists for rushing students through elementary mathematics in order that they may in the shortest possible time reach physics, both experimental and mathematical, the necessary foundations of scientific knowledge are seldom properly laid. Boys who ought to be learning skill in Algebraical manipulation, in assimilating Trigonometrical formulæ, and in applying them to various problems of Mensuration, are, I find, endeavouring to limp through Statics, Hydrostatics, and Kinetics. When to these comparatively advanced subjects we add some Chemistry, the phenomena (at least) of Heat, Optics, Sound, Electricity, and Magnetism—to say nothing of languages—the result is inevitable that the less showy subjects of elementary pure Mathematics must be insufficiently studied—must, in fact, be merely skimmed.

There is no subject in which the result of this over-haste is so easily recognized as Trigonometry; for, at the outset, the student's work in this branch must largely consist in committing to memory a number of formulæ, and nothing but long-continued practice in application will fix them in the mind. Hence, as the necessary time must be given to several other subjects, I find very many students exceedingly slow in repeating, and even in recognizing, some of the most elementary and frequently useful formulæ in Trigonometry. Hence also a large portion of the knowledge brought out in competitive examinations consists of what is called "Cram," and it is, therefore, customary to heap odium on the "Crammers." I do not think, however, that the fault rests with the Crammers, who do merely what they are invited to do by educational authorities.

I shall take as an instance of the excessive haste with which students are pushed on through various branches of Science the Matriculation Examination of London University; and what I say with reference thereto is the result of a present experience which I have in assisting a young relative in his reading for this examination. In last year's Regulations for Matriculation you will find that the course of Mathematics consists of Arithmetic, Algebra as far as easy quadratic equations with questions involving their use, Geometry to the extent of the first four books of Euclid, with simple deductions; and in Mechanics the requisites are "elementary notions as to Velocity, Acceleration, Force, Mass, Momentum, Work, and Energy, Composition and Resolution of Velocities, Accelerations, and Forces in one plane. Moments and Couples in one plane. Centre of Gravity, or Mass-centre. Transmission of Pressure in Liquids; variation with depth of the pressure due to weight of liquids. Specific Gravity and modes of determining it. Pressure of gases, and laws relating thereto. Atmospheric pressure. Common instruments and apparatus whose action depends upon the pressure of liquids, or of the atmosphere, or both." In addition to this, the candidate must take up either Chemistry, or Heat and Light, or Electricity and Magnetism.

Now you will observe particularly two things about this prescribed course. Firstly, the candidate is not supposed to have any knowledge of the fifth and sixth books of Euclid, and therefore no knowledge of the propositions relating to the ratios of linear or other magnitudes; and, secondly, that all knowledge of Trigonometry is excluded.

[1] A Paper read before the Association for the Improvement of Geometrical Teaching, January 19, 1889, by Prof. Minchin, President.

What boy with this slender knowledge of pure Mathematics is likely to have a correct notion of the nature of Acceleration? And, without knowing the meaning of a sine or a tangent, to what can his knowledge of the Composition and Resolution of Forces and of the method of taking Moments amount?

A course of reading in Mechanics and Hydrostatics, specially constructed so as to avoid Trigonometry and the sixth book of Euclid, is a curious object to contemplate. It reminds one of a number of large boulders thrown out into a stream, with painfully and dangerously wide intervals between them, their tops barely above the water, to enable a man to get across by a series of courageous jumps; and, as in the case of such a traveller, not all the caution nor the acrobatic deftness of balancing on the point of one foot which he possesses will prevent him from tumbling off his flimsy and rickety supports, so in the case of the student for whom knowledge has been packed up into a number of scientific boluses, it is impossible to avoid the acquisition of erroneous notions and fallacious rule-of-thumb principles, which, assimilated thus early, have a strong tendency to remain rooted in the mind.

In the case of my young friend I found it quite impossible to impart anything that could with propriety be called a knowledge of Mechanics without the aid of Trigonometry. In the Composition and Resolution of Forces a few questions made specially to order, which usually depended on the facts that the sine and cosine of 45° are equal, and that the sine of 30° is $\frac{1}{2}$—although, of course, the mention of a sine or cosine was inadmissible—exhausted the field; and the same restrictions were imposed on the treatment of Moments, so that I was obliged to abandon the task, and to insist on a small knowledge of Trigonometry and the sixth book.

In such a cramped and stunted knowledge there is nothing of spontaneity, nothing of power, but much of danger. It is far better to give no encouragement to it, to defer the attempt to study the elements of mathematical physics until the old and well-recognized branches of elementary pure mathematics have been studied with some thoroughness.

The want of thoroughness which seems to me to be so prominently characteristic of our works on mathematical physics sometimes exhibits itself, quite unconsciously on an author's part, in a ludicrous manner. It may, perhaps, be best described as "Calculus dodging." For some curious reason, which I have never discovered, it has been generally assumed that a student can possess a very extensive knowledge of the results and principles of Dynamics—of the composition and resolution of forces and couples in three dimensions, of the principle of work and energy, of the nature and properties of tubes of force, Potential, &c.—without any knowledge of the Differential or Integral Calculus. This is, surely, a piece of self-deception. The processes of differentiation and of elementary integration are not difficult of acquirement, and it seems to me that they ought to be studied before such an extensive inroad is attempted into Dynamics. But, presuming that such knowledge is not possessed by the reader, we find the author performing such an integration, for example, as that of $\frac{dx}{x}$ between specified limits by a process which must strike an intelligent student as at once most ingenious and most unnatural. Special devices exhibited "for this occasion only" confer no independent power on the student, and, moreover, require him to possess an amount of ability which would be much better and more successfully employed in acquiring a knowledge of the principles of that Calculus which is thus evaded by artifice.

In all such cases it may be said, I think, that a student who is capable of understanding the notions involved will arrive at the results by other than the special artificial methods employed by the author; while in the case of a student for whom such methods are necessary, most probably the notions are unsuitable, and the method of proof is apt to puzzle him with what seems to be mathematical jugglery.

If more time were spent in teaching the mathematical principles on which quantitative physics depends, there would be less need for such methods, and in the long run the student of physics would be a gainer.

But, while advocating a more thorough and leisurely study of the elements of pure mathematics before the study of physics, it seems to me that many of our elementary text-books in mathematical physics make the mistake of occupying the student's attention with questions which, being far removed from physical reality, and being, in fact, merely disguised mathematics, had much better be omitted. We should make the effort to make our works on physics as physical as possible, to use Mathematics for the sake of Physics, and not *vice versâ*; and it is time to recognize that the field of physics is now so extensive as to supply subject-matter for calculation and for illustration of mathematical principles, and to permit us to curtail greatly the space which is now devoted to things comparatively useless.

Let me take one or two examples. Is anything gained by teaching students the very numerous properties of the curve which a particle would describe if it were projected with any velocity, under the influence of gravity, if the Earth had no atmosphere?

Again, let us turn to any work on Hydrostatics, and we are certain to find a very large number of mathematical trivialities. I say nothing of a number of liquids, whose densities are in some kind of progression, superposed in very fine tubes of peculiar shapes. These are harmless; but what about that notion of *whole pressure* on a curved surface—the sum of all the normal pressures, or, more strictly, the surface-integral of pressure over the curved surface?

Numerous, indeed, are the mathematical problems to which this notion gives rise; but what about the physical idea involved? It is easy to comprehend the trouble which a teacher lays in store for himself if he practises his students in the process of adding together the magnitudes of a number of forces whose lines of action are in all directions in space. And remember that, in most cases, very few students who are taught to calculate the whole pressure of a fluid on a curved surface will subsequently learn that a similar process has a physical meaning only in the case of the normal flux of Newtonian gravitation through a curved surface. Let us see, however, the physical meaning which is actually attributed to the whole pressure of a curved surface by the author of one of our very elementary text-books for students. The following is the literal statement:—"It should be observed that these pressures act in different directions, the pressure at each point being perpendicular to the surface at that point. The whole pressure is the sum of all these pressures, and represents the total strain to which the vessel containing the fluid, or the body immersed, is exposed." Now in this statement, besides the addition of the magnitudes of a number of non-co-planar forces, we have the misuse of the term *strain* and the completely unintelligible expression "total strain" of the vessel. And, in illustration of this definition, we are given the following example: "Show that if a sphere or a cube be filled with liquid, the total strain to which it is subjected is three times the weight of the liquid it contains." You will, I hope, agree with me in holding that such teaching is in the highest degree erroneous and objectionable, and that the efforts of those who have to teach should be directed against it.

So far as my observation goes, the principle making Physics a mere disguise—and a very unskilful one—for pure mathematics is much too largely adopted by writers

on mathematical physics, with the result that many students receive very insufficient practice in acquiring and developing physical conceptions.

To this cause, also, must be attributed the fact that proficiency in mathematical physics is a much rarer thing than proficiency in pure mathematics, the former being made, to such a great extent, subservient to the latter. Indeed it was not, I think, until the publication of Thomson and Tait's "Natural Philosophy" that the ancient problem-grinding system, which hindered the progress of exact Physics, received a severe blow, and Cambridge, under the influence of the ideas and methods of Thomson and Clerk Maxwell, produced a plentiful supply of physicists.

But the good that was effected by Thomson and Tait is confined almost entirely to advanced students; in the hands of those of a lower standard are still to be found text-books of the old sort, teaching Mathematics under the guise of Physics, presenting nothing but the dry husks of the latter, and, by inaccurate language, laying an early foundation of erroneous notions. Some people of the older school—not a few—express, indeed, a strong impatience with us if we protest against the use of slipshod and inaccurate language in our text-books. They tell us that we are doing the matter of nomenclature to death, and that, after all, we need not be very particular about the choice of exact terms. This is a most unfortunate attitude; it is one, however, which this Association has done, and is doing, something to counteract. Surely it must be admitted that if the conceptions of Physics are presented to the beginner in erroneous language, there is a danger that in many instances these conceptions will never be properly acquired. And is not accurate language as cheap as inaccurate?

I know of several text-books still in the hands of schoolboys and others in which, in whole pages of answers to problems on the motion of a particle, *velocity* is always spoken of as so many feet, and *acceleration* also as so many feet—no reference being made to *time*, and the fact that acceleration has a double reference to time being never mentioned. I assume it as evident that the progress of such students is seriously hindered and delayed by such teaching.

If the writers of such text-books as those to which I refer have really clear and correct notions themselves, they ought to take pains to present them accurately to beginners who have to learn from them. Not to do so is to ignore the fact that a great deal of the difficulty of learning any branch of science is removed if only the student is started upon it with clear and correct notions as to the various entities with which the science deals, and with its fundamental principles expressed in accurate language.

Let us take, as another example of exceedingly erroneous teaching, the following exercise—one of a great many, all similarly expressed—from a text-book still in the hands of beginners: "The time occupied by a body in describing uniformly a complete revolution in a circle, whose radius is 11 feet, is 16 seconds, calculate the centrifugal force which acts upon it. *Ans.* 1·696 feet per second."

In this we have two bewildering errors instilled into the student's mind, viz. that *force* is measured in feet per second, and that when a body is revolving in a curve, it is acted upon by an *outward* normal force—the old time-honoured fallacy about centrifugal force. It is almost miraculous that any accurate scientific knowledge is acquired in spite of such fallacious teaching as this.

I wish to emphasize this teaching about *centrifugal force*, because, no matter how often it is condemned and shown to be erroneous, it still flourishes in our scientific text-books. In another elementary work which is extensively used, we find the nature of centrifugal force more fully explained in the following manner. "In order to keep a body moving in a circle, there must be a force acting upon it which will produce a constant acceleration towards the centre, equal to $\frac{v^2}{r}$. Hence, if W be the weight of the body, P the pressure tending towards the centre, $P = W\frac{v^2}{gr}$." This is, of course, all right; but then follows the mysterious statement, "a pressure equal and opposite to P is sometimes spoken of as the centrifugal force."

I call this a mysterious statement, because it does not tell us on what this pressure equal and opposite to P acts, or whether it really acts on anything at all or not. We know, of course, that this reversed force is the reaction of the moving particle on the agent or surrounding medium; but how is a beginner to know this? Will he not naturally fall into the error of supposing that it acts on the moving body itself? Or, if he possessed a little intelligence, would he not ask, "Why do you require me to think of a force equal *and opposite* to one which acts on the body? I have nothing to do, when considering the motion of the body, with any forces except those which act on the body itself; and why do you not ask me to consider a force equal and opposite to some of the other forces acting on the body—a force equal and opposite to its weight, for instance? It would be just as sensible to do so, since this reversed weight is the body's reaction against the Earth."

The only true reply to such a student is to confess that it is quite a mistake to introduce the conception at all; but instead of this, we find that the discussion proceeds to encourage the erroneous notion that centrifugal force acts on the moving body by taking the case of a carriage describing a curve of small radius.

Now, what is it that has given rise to this fallacy about centrifugal force? Is it the fact, so often adduced, that, if we tie a stone to one end of a string and, holding the other end in the hand, whirl it round, we feel an outward force pulling the hand? But then, if we imagine the stone to be attached to an elastic string, one end of which is tied to the hand, while the stone is projected vertically upwards, the hand would experience an *upward* pull; and are we thence to conclude that the stone is continually acted upon by an *upward* force?

Possibly the whole fallacy is traceable to D'Alembert, who gave us a sort of dynamical *memoria technica*, usually expressed in the words, "forces equal and opposite to the effective moving forces of a material system are in equilibrium with the external forces,"—a very imperfect and misleading statement for the principle obviously contained in Newton's first and second axioms, viz., "the mass-accelerations of the particles of any material system have at each instant *the same* total component along any line, and *the same* total moment round any axis as the external forces acting on the system."

D'Alembert's fictitious reversal is unnatural and unnecessary; and, in introducing fictitious forces and reducing the state of the moving material system to one of equilibrium, he ignored the actual state of affairs. He should have faced the state of motion as it exists, and recognized the fact that the production and maintaining of a given state of motion requires the action of a definite system of forces assisting the motion, instead of concentrating his attention on something which would stop the motion. It is this determination to fix the mind on a state of equilibrium, and to ignore the actual motional state that is, I think, responsible for the fallacy of centrifugal force.

[Other examples of erroneous teaching with regard to centrifugal force, the confusion of *work* with *horse-power*, &c., were then given.]

We must remember, also, that a teacher who has often to correct the erroneous language of the text-book which he employs with his class is by no means certain of inspiring confidence in himself and convincing his pupils that the teaching of the author is at fault; for, young students, very unlike Prince Bismarck, have a profound

respect for printer's ink; hence they are always disposed to believe that the book is right, and if you tell them that an acceleration is not so many *feet*—as the book says it is—but so many *feet per second per second*, they accept the correction for politeness or for peace sake, but not with any conviction.

In addition to numerical calculation as an important aid in the teaching of Mathematical Physics, I would also advocate the employment of *graphic methods of solution* in cases in which exact solution is not possible. The graphic method of solution is scarcely recognized at all in our text-books. Every text-book of Statics gives us, indeed, its modicum of graphic representation, which is usually exhausted in a bare and barren exposition of the principle of the *triangle of forces* and the *polygon of forces*, which has long since become stale "book-work."

But this is by no means what I advocate. I intend graphic solution at once as an aid to calculation, as a means of strengthening the student's interest in the subject, and as a representation to the eye of the possibilities and limitations of any particular problem under discussion.

[The problem of the equilibrium of a camp-stool was then given as an illustration of the graphic method.]

In Statics and Hydrostatics we have abundant instances of this kind, and their solutions by the graphic method furnish excellent practice for the ingenuity of the student, giving to each problem the interest which a vivid picture always gives, as well as strengthening his knowledge of the results and methods of pure Mathematics.

The sum total, then, of what I have said with reference to our elementary text-books comes to this—that, while we are abundantly supplied with high-class scientific works of an advanced character, a corresponding improvement has not generally taken place in the books from which school-boys and other beginners have to learn; that these books are often marred, not only by inaccurate language and fallacious teaching, but by a certain scrappiness in their mode of treatment which is encouraged by the desire of examining bodies to hurry through and skim over a large number of scientific subjects at the expense of a more leisurely study of the foundations of Science.

I should like, in conclusion, to say a few words with regard to the way in which the cramming of Science is encouraged by examinations.

Wherever well-worn book-work is set at an Examination, it is extremely likely that cram will find its opportunity, and it may be impossible for an examiner to detect it. Assuming that book-work must be set, I do not know of any reliable safeguard against cramming except *vivâ voce* examination. But a *vivâ voce* examination which is made a part of the competition, and in which the candidates do not all get the same questions, is, I think, essentially unfair. Whenever such an examination is possible, its function should be to discuss with each candidate the several questions with which the written paper deals. In this way, when his knowledge of any particular question or method appears to the examiner of the written paper to be doubtful, the doubt is very speedily settled one way or the other. But, in the absence of such a corrective, an examiner who is, after all, dissatisfied with the candidate's working of any particular question, and yet disinclined to allow him no credit whatever, is obliged to resort (at least in many instances) to some systematic method of cutting off marks, and this method is not infrequently a system of elaborate trifling.

As a particular example, take a question which is sometimes set at examinations in Science—the method of determining H, the horizontal intensity of the Earth's magnetic force. A part of the process consists in the discussion of the vibrations of a uniform magnet bar round a vertical axis under the influence of the horizontal magnetic force. Such a bar is a compound pendulum, and its motion involves a knowledge of its moment of inertia and the integration of a differential equation of the second order. If at any instant θ is the angle made by the axis of the bar with the magnetic meridian, the expression $\frac{d^2\theta}{dt^2}$ is involved in the equation.

Now, I know a case in which the whole of this process was crammed up by a candidate who, although he had a good practical knowledge of Physics, did not know how to resolve a force along a line, did not know the meaning of the moment of a force about an axis or of the moment of inertia of a body about an axis, and, of course, was wholly ignorant of the meaning of the expression $\frac{d^2\theta}{dt^2}$. Indeed, this last was a puzzle to him, for he always failed to place the two figures "2" in their proper places. Yet the whole thing was crammed up with sufficient success to obtain credit for the question.

In such a case as this an examiner may have very grave suspicions, but he may be obliged to give a large measure of credit, nevertheless. Frequently the method of marking such an answer is something like this—the expression $\frac{d^2\theta}{dt^2}$ involves two d's, two 2's, a t, and a θ; if the candidate uses all these symbols, each in its proper place, give him full marks; if he uses all the symbols, but *one* is misplaced, take off *one* mark; if *two* are misplaced, take off *two* marks; and so on. This is of course an exaggeration, but it is the *kind* of system adopted—necessary, perhaps, but extremely unsatisfactory.

Now a *vivâ voce* examination of the candidate on this question would, in a few seconds, have decided his mark to be *zero*.

In cases in which the number of candidates is very large, it would seem to be impossible to apply a *vivâ voce* test; but there are cases in which it could be used, and used, I have no doubt, with good effect.

The setting of book-work at examinations is, I think, much too frequent. It gives a hope of success to candidates who really have no vocation for a subject, but who load their memories sometimes with whole pages of work of which they understand almost nothing.

Our examination system has its defects; but these defects are almost wholly due to the shortcomings either of individual examiners or of the directors of education.

I have spoken of the excessive haste with which educating bodies compel students to attempt the acquisition of a large and varied assortment of knowledge; but I must not omit to add that the evils of examination are greatly aggravated by the prodigious haste which is imposed on examiners themselves in some public competitions. Nothing can justify a system which requires an examiner to read over and determine the numerical value of every one of 1200 papers in little more than a fortnight. The thing is utterly impossible, and the results must be untrustworthy.

But, whatever the defects of the examination system may be, I hold that the *principle* of examination is good.

[Some of the objections of the writers in the *Nineteenth Century* were then dealt with in detail. Referring to the unsatisfactory method by which professors and lecturers are now appointed, Prof. Minchin continued:—]

Mr. Frederic Harrison says, "Trust the teacher; trust him to teach, trust him to examine;" and I should say that, if we did, we should find our trust far more often misplaced than it would be by trusting wholly to the independent examiner. If the teachers were always people who could teach, the case would be different; but what is the method of appointing teachers, as a rule, in England? People make inquiries as to what scholarships, prizes, and degrees have been obtained at the University by a candidate for a teaching post; but of his capacity for imparting successfully any of his own knowledge to others there does not, in most instances, exist the slightest proof; and hence we are often presented with

such lamentable teaching failures. Under these circumstances it would be ridiculous to "trust the teacher, trust him to teach, trust him to examine;" for, bad as examiners are, teachers are worse—partly for the same reason as that for which white sheep eat more than black, viz. that there are more of them.

And is it not also vain to attempt to replace the Examination system, even at the Universities, by a system of Thesis writing on some particular subject selected by the candidate, the Thesis being written by him at home, with ample opportunity for composing it out of works of reference, or with the assistance of his friends, with the addition of a small make-believe of "original research?"

We are told by one of the writers in the *Nineteenth Century* that, under our Examination system, England is losing her intellectual giants, men who are a head and shoulders above their contemporaries; but I ask is the writer aware that we have still among us Sir William Thomson, and that we have only recently lost Clerk Maxwell? People who speak thus seem to think that we are to expect a crop of intellectual giants to sprout up like a crop of mushrooms, and to be ready whenever we want them.

I would say in conclusion—let us retain Examinations, make them tests of a knowledge of the applications of principles, so that they will be real tests of intelligence, and not of cram; do not make them so difficult and so high-flown as they often are; and, finally, do not encourage the hurrying and skimming through a large number of different subjects, but make sure of the foundations by a more thorough and leisurely system of study.

THE LIFE-HISTORY OF A MARINE FOOD-FISH.[1]

I.

IT is but a few years since the life-history of our most important marine food-fishes was involved in considerable obscurity—not only as regards popular views, but even in respect to the knowledge of men of science. Thus, for instance, in the years 1883 and 1884 the almost unanimous opinion of British fishermen was that our common food-fishes sought the shallow water of the bays and inshore ground generally for the purpose of depositing their eggs on the bottom. No observations specially bearing on this point had been made by British zoologists, and a series had to be undertaken for a public inquiry then in progress—with a result which demonstrated how extensive the reverse of the popular notion was. Again, certain comparatively recent authors on British fishes speak of a common fish like the gurnard as spawning twice a year, whereas, after careful observation, no evidence in support of this view has been obtained. The same obscurity veiled the larval and post-larval conditions of most of the food-fishes, even G. O. Sars—in regard to the latter stage—describing no intermediate forms between the larva of 6 mm. and the post-larval stage of 24 mm. in the cod, almost the only fish to which some attention had been paid.

On the other hand, our knowledge of the development and life-history of the fresh-water fishes—such as the salmon, trout, and charr—has for many years been well understood—thanks to the labours of Louis Agassiz and Vogt in Switzerland, Coste and Lereboullet in France, Ransom in England, and Shaw in Scotland, on the scientific side, and of the noblemen and gentlemen of Perthshire (ably seconded by Robert Buist) in connection with Stormont Field Ponds on the Tay, on the popular side. Much information has also been recently obtained by Dr. Day and Sir J. Gibson Maitland at the excellent ponds of the latter at Howietoun.

[1] A Discourse delivered by Prof. W. C. McIntosh, F.R.S., at the Royal Institution, on Friday, February 1, 1889.

A short time ago, relying on experience derived from fresh-water fishes, not a few imagined the eggs of marine fishes as readily visible and tangible objects—possibly associated in their minds with certain practices in trout-fishing, or it may be with the manufacture of caviare. Recent investigations, however, have shown that in most marine food-fishes the eggs are minute glassy spheres which float freely in the ocean. For a knowledge of this fact we are indebted in the first instance to Prof. G. O. Sars, of Christiania, a naturalist trained from boyhood under a distinguished father, and who, by a fortunate appointment to a fishery post in Norway, was enabled to discover that the eggs of the cod, haddock, and gurnard, floated in the water, or, as we term it, were pelagic. He thus opened up a new field in the economy of the food-fishes, which in a great maritime country like ours ought not to have remained so long unexplored.

Lately, however, attention has been earnestly directed to the subject, and the labours of Cunningham, Brook, Prince, and others have made considerable advances in this department.

It is now known that the great majority of our British marine food-fishes—indeed, all our most valuable kinds (including even the sprat and the pilchard amongst the Clupeoids) produce minute eggs—as transparent as crystal, and which float freely throughout the water. These eggs, moreover, are not all shed at once, as in the case of the

FIG. 1.[2]—Pelagic egg of the ling (enlarged).

salmon, but successive portions of the ovary become ripe, and the eggs then issue externally. If by any accident or irregularity—as for instance the confinement of a flounder in an unhealthy tank—this gradational issue is interfered with, the animal dies from the great distention of the body caused by the pent-up eggs. In the case of the cod this gradual issue of the eggs continues probably for a week or two, so that the progeny of a single fish in one season may vary considerably in size.

From the early months of the year onward to late autumn the sea off our shores thus abounds with pelagic eggs, those of the rockling, haddock, and sprat being amongst the earlier forms, while the later include those of the sole. As indicated in the Trawling Report, and now supported by further experience, it would be a very difficult matter indeed to arrange for a close time in the sea—that is to say, for a limited period during which the mature fishes might be permitted to spawn in peace. This, however, in the case of individual species, such as the cod, might more readily be carried out, so as to save the mature fishes at the spawning period.

In a vessel of still sea-water these transparent glassy spheres rise at once on issuing from the fish, and form a stratum on the surface. Even the ripe portions of ovaries removed from the rejected viscera on a pier will show the same features, and thus, indeed, they first came before the lamented Lord Dalhousie at Anstruther. In the sea, however, they are seldom met with on the surface, and the tow-nets require to be sunk a fathom or two for their capture, their specific gravity being so little less than that of sea-water that they are carried hither and thither by the

[2] I am indebted to Mr. E. E. Prince, B.A., for kindly aiding me with sketches for the woodcuts. The sketch of *Motella* is by Dr. Scharff.

currents in every direction. Some indeed are captured near the bottom by nets attached to the trawl-beam, while experience with the large net of the St. Andrews Laboratory has proved that a great number are carried in mid-water.

When these glassy eggs issue from the female fish, they are soon fertilized in the surrounding water; so that in British waters, at any rate, non-fertilization is one of the rarest conditions in these pelagic eggs. It is indeed more likely to happen in the case of the herring, which deposits its eggs in masses on the bottom, or in artificial circumstances in tanks. The unfertilized egg soon becomes opaque and sinks, so that it is readily recognized.

In this connection I would again refer to the notion not long ago firmly rooted in the minds of many—especially those practically engaged in fishing—that the fishes at the spawning season seek the shallow water in which to deposit their eggs. Now there is little in Nature to support this idea. Shore-fishes, it is true, such as the lump-sucker and sea-scorpion (*Cottus scorpius*), do deposit their eggs there (and there cannot be a doubt that some of the masses of eggs thus deposited have been mistaken for those of the food-fishes); but the edible fishes proper, such as the cod, haddock, whiting, flounders, and others, appear to produce their eggs just where they happen to be feeding at the season. Their eggs are taken in charge by the ocean generally, and hence are independent of any imaginary protection or privilege pertaining to the shallow waters.

Moreover it does not follow that the fishes of an inclosed bay[1] will increase of themselves. As in the case of the plaice, in shallow sandy bays, it may happen that most of the large mature female fishes are beyond the limits, the half-grown or immature forms mainly occurring within; pelagic ova therefore must be borne inward, and still more the pelagic young, while the post-larval stages likewise migrate shorewards; a counter-migration of the older forms subsequently taking place to the deeper water. Such bays, therefore, have to depend for their stock of fishes on the unprotected off shore. If by any chance the latter waters were depopulated, the inshore would seriously suffer.[2]

The minute size of the eggs of all the important marine food-fishes enables a fish like the cod, for instance, to produce an enormous number—probably about 9,000,000, as against the 18,000 to 25,000 of the salmon or the 10,000 to 30,000 of the herring, both of which fishes deposit their eggs on the bottom. In the same way the very small eggs of the dab provide for a large annual increase of the species.

The translucent eggs, which, unless they contain a globule of oil, as in Fig. 1, are difficult to see in some instances even in a glass vessel, thus escape (by floating throughout the water) the vicissitudes to which a purely surface-life would expose them, such as the admixture of the surface-water with rain, and the attacks of gulls, ducks, and other forms; and they also are less at the mercy of the active predatory races living on the bottom, not to allude to the risks of being swept by storms on the beach or captured and destroyed by the ground-rope of the trawler. Nature, indeed, could have devised no method more secure than this for the safe propagation of those valuable fishes which for ages have peopled our waters, and I venture to say, with Prof. Huxley, will perhaps people them for ages yet to come, notwithstanding the persistent efforts of man to annihilate them.

Some good observers, for example Prof. Ryder in America, have attached much importance to the oil-globule in eggs which are pelagic, but its buoyant influence has been slightly over-estimated, for some contain no oil-globule, while the massive oil-globules in the eggs of the salmon and cat-fish have no such effect. They float, as well shown by my friend Mr. Edward E. Prince, Secretary to the Mussel and Bait Committee, solely in virtue of their specific gravity, which is somewhat less than that of sea-water. The moment fresh-water is added they sink, as they likewise often do when transferred from a vessel filled at sea into one containing shore-water.

[1] For example, closed by a Fishery Order.

[2] This feature was pointed out in the Report of H.M. Trawling Commission, under Lord Dalhousie.

While immediately after deposition these minute spheres are prone to accident from impurity and sudden changes in the temperature of the water, such would not seem to be the case after development has made some progress. Thus many living eggs will be found in odoriferous vessels brought from sea by the fishermen if the inclosed embryos have reached an advanced stage. Again, while carrying out some experiments on temperature (at the suggestion of Prof. Huxley) during the trawling expeditions, I had occasion to heat a test-tube containing some of the eggs of the flounder, so as to make them rush up and down the vessel most actively. Considerable heat was applied, and, under the impression that the eggs were irretrievably injured, the tube was set aside. Some days afterwards, when explaining the nature of the experiment to Prof. Ewart, he noticed motion in the tube, and further examination showed that after all this exposure to heat the little flounders had emerged as usual, and were alternately floating and swimming about in the water. On the other hand, severe frosts are fatal to ova crowded in shallow vessels, in many cases actual rupture taking place;[1] and the same occurs in large eggs, for example those of the cat-fish deposited on the bottom of the vessel.

Out of the little glassy sphere, after a longer or shorter interval (varying from a few days to a few weeks, according to temperature), comes a minute and nearly transparent fish which at first is often as passive in the currents as the eggs themselves.[2] It soon, however, uses its tail for swimming and its pectoral fins for balancing. Its shape

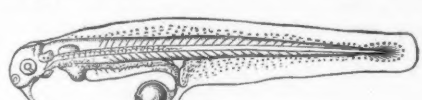


FIG. 2.—Larval ling immediately after hatching.

is somewhat like that of a tadpole, partly from the large head, but mainly from the great size of the yolk-sac, which contains a store of nourishment on which the little mouthless creature, about 3 mm. long, sustains itself for a week or ten days. In this respect it somewhat resembles the young salmon, in which a much larger collection of the same food supports it for about six weeks amongst the gravel in the spawning-bed of the river, though a closer scrutiny reveals certain essential differences. Thus the store of nourishment in the yolk-sac of the salmon is taken up by the blood-vessels which branch in a complex manner over the whole yolk, whereas in the young cod, though the heart is present and pulsating, not a blood-vessel at first is seen, and none ever enters the yolk-sac. The absorption of this nourishment therefore must take place by aid of the cells and tissues themselves, and there is nothing specially wonderful in this, when the condition in the endoderm of *Hydra* and other instances of intracellular digestion are considered.

It has been mentioned that these minute and most delicate little fishes are nearly transparent, and this is more or less the case throughout, though in the majority—even before they leave the egg—points of pigment appear here and there in the skin, so as to give them a distinctive character. After hatching, these pigment-spots branch out in a stellate manner, thus becoming more evident, and it is found that in most cases each little food-fish has

[1] NATURE, June 1886.

[2] For some years the development of fishes has been studied by able workers: amongst others, on the Continent, by Götte, Kupffer, Hoffman, Henneguy, E. Van Beneden, Osjannikov, and Rafaele; in America, by Alex. Agassiz, Ryder, and Whitman; while in our own country, Ransom, Klein, Cunningham, Prince, and Brook have carried out similar researches.

colours of its own. Thus the cod is known by its four somewhat regular black bands, the pigment on the haddock being less defined, the whiting by its canary-yellowish hue, the gurnard by its chrome-yellow, the ling by its gamboge-yellow, the flounder by its yellow and black, and

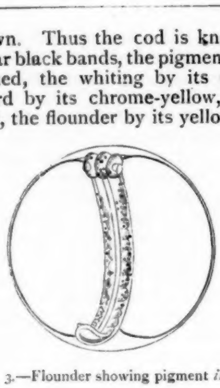


Fig. 3.—Flounder showing pigment *in ovo*.

so on. All these hues, however, become greatly modified during subsequent development; indeed, the pigment in no group of vertebrates shows more remarkable changes between the young and adult states than certain of our food-fishes. Thus for instance the cod is characteristically

Fig. 4.—Larval cod with black spots or bands.

speckled in its tiny youth (Fig. 4), next it becomes more or less uniformly tinted, then the pigment groups itself somewhat irregularly on the sides (Fig. 5); thereafter it is boldly tessellated, subsequently blotched with reddish-

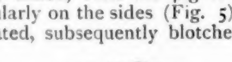


Fig. 5.—Aggregations of pigment in post-larval cod.

brown, and finally in its adult condition it again puts on more or less uniform tints. The ling shows a similar series of transformations, the colours, however, differing in their arrangement, being marked with gamboge-

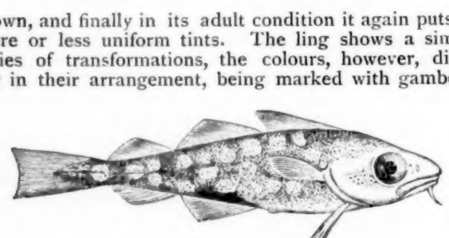


Fig. 6.—Tessellated condition of young cod (spirit-preparation).

yellow in its larval, slightly banded in its early post-larval stage, then the body becomes more or less uniformly tinted in its post-larval phase, and the little fish is furnished with a pair of enormously developed and bright yellow

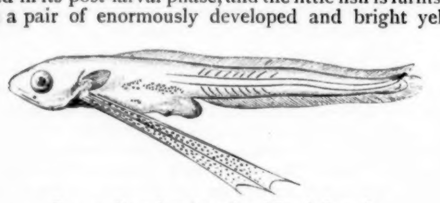


Fig. 7.—Long-finned post-larval ling (enlarged).

ventral fins (Fig. 7)—so different from the short ones of the adult. It is next striped conspicuously when about 3 inches long (Fig. 8), thus affording a great contrast to the tessellated condition of the young cod. In this stage an olive-brown band passes from the tip of the snout in a line with the middle of the eye, straight backward to the base of the caudal fin-rays. The pale ventral surface bounds it inferiorly, while a dorsal stripe with a beautiful opaline lustre runs from the tip of the snout, over the upper part of each eye to the tail, on which it is opaque white, thus giving the fish a characteristic appearance. The dorsal line from the brain backward is distinguished by a narrow edge of dull orange or pale olive, which relieves the colours formerly mentioned, and the general effect is varied by two black specks in the dorsals. When it is double the length (*i.e.* 6 or 7 inches), a complete

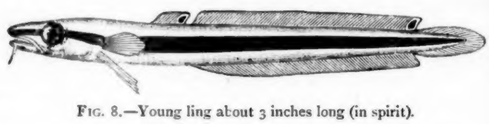


Fig. 8.—Young ling about 3 inches long (in spirit).

change has taken place in its coloration (Fig. 9). Instead of being striped, the fish is now boldly and irregularly blotched—both dorsally and laterally, the region of the white stripe being indicated by the pale and somewhat scalloped area dividing the dorsal from the lateral blotches. Fourteen or fifteen brownish blotches occur between the pectorals and the base of the tail, and they are separated by the whitish areas, which thus assume a reticulated appearance, and both kinds of pigment invade the dorsal fins. Other touches of dark pigment on the fins and tail increase the complexity of the coloration at this stage.

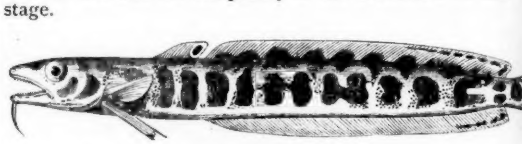


Fig. 9.—Young ling in the barred stage (about half natural size).

Again, some species, like the gurnard, have pigment over the yolk-sac before they are hatched, others have not. The dragonet in its post-larval (and pelagic) stage has its ventral surface deeply tinted with black pigment, while in the adult (a ground-loving fish) it is white. The St. Andrews cross in the eye of the post-larval four-horned Cottus (*C. quadricornis*) is another remarkable feature (Fig. 10). No more interesting or more novel field, indeed, than this exists in the whole range of zoology; but the investigations need ships and boats, with expensive appliances, as well as persevering work for several seasons. We have only been able to open the field at

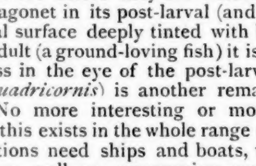


Fig. 10.—Head of *Cottus quadricornis*, with St. Andrews cross in eyes.

St. Andrews by the help of the Trawling Commission under Lord Dalhousie, and subsequently by the aid of the Fishery Board. It may be asked, Why is all this remarkable variation in colour? Just for the same reason that the young tapirs and wild pigs are striped, or the young red deer spotted—the adults in each case being uniformly tinted. Such features indicate their genetic relation with ancestral forms having these marks; and, moreover, in the struggle for existence, such variations in tint conduce to the safety of the young.

The view of Eimer that the markings in animals are primitively longitudinal would not suit for many fishes,

notably for the young cod, ling, and Pleuronectids, and, indeed, Haacke has already pointed this out from a study of the Australian fish *Helotes scotus*,[1] the adult of which is marked by eight longitudinal bands, while young specimens present in addition a row of clear transverse bands which subsequently disappear.

(*To be continued.*)

NOTES.

On account of the severe illness of his child, Prof. Mendeleeff was obliged to leave London early on Tuesday morning, very greatly to the regret of those who assembled in the evening to hear him deliver the Faraday Lecture, which was read by the Secretary. It was announced that the dinner, at which the Fellows of the Chemical Society proposed to entertain Prof. Mendeleeff on Wednesday evening, would not take place, but that the President, Dr. Russell, and Miss Russell would receive at the Grosvenor Gallery on Friday evening, as before arranged.

The Ladies' *Conversazione* of the Royal Society will be held on Wednesday, June 19.

The *conversazione* of the Society of Arts will take place at the South Kensington Museum on Friday, June 28.

It has been decided that a statue shall be erected in honour of the late John Ericsson in Stockholm. His biography, papers, and letters will be edited by one of his most intimate friends, Colonel Church, of the American *Army and Navy Journal*.

Dr. George Owen Rees, F.R.S., died at Mayfield, Watford, Herts, on May 27. Dr. Rees took his degree of M.D. at Glasgow in 1837, and became a Fellow of the Royal Society in 1843.

It is requested that all persons having in their possession letters from the late President of Columbia College, Frederick A. P. Barnard, will be kind enough to send them to Prof. Nicholas Murray Butler, Columbia College, New York, U.S.A., at their early convenience. The letters will be returned after copies have been made.

Sir Lyon Playfair, who recently resigned the secretaryship of the Royal Commission of the Exhibition of 1851, has been succeeded by Major-General Ellis, Equerry to the Prince of Wales.

Allen's Indian Mail reports that Dr. Burgess, Director of the Archæological Survey of India, will come home immediately, and that he is retiring from his appointment.

The foundation-stone of the Framjee Dinshaw Petit Laboratory of Scientific Research, in Bombay, was laid on April 8 by Lord Reay. Mr. Petit, the son of the donor, explained that it had appeared to his father desirable, in the interests of medical education, that a laboratory for scientific research in biological and physical sciences should be established. He had long cherished the wish to have the properties of Indian drugs investigated, and made known to medical students. The laboratory, which will be connected with the Grant Medical College, was described by Lord Reay as the only missing link in the educational programme he had sketched out for Bombay.

The Allan Line steamer *Caspian*, which arrived from Halifax at Queenstown on Tuesday evening, reported having passed in the North Atlantic no fewer than thirteen large icebergs. The ship steamed quite close to one of them on Thursday last.

We are glad to be able to report the publication of synoptic weather maps twice a day by the Central Physical Observatory at St. Petersburg, beginning with May 12. The stations for which information is given extend from the west of Ireland to the Ural Mountains and the western shores of the Caspian, and from the north of Norway to the Trans-Caucasus, showing the weather-conditions at a glance, over nearly the whole of Europe. The atmospheric pressure is represented by isobars, and other elements by figures and symbols; the charts being printed on the back of the tabular reports, which have been issued since 1872. A general summary of the weather is given in Russian and French.

[1] One of the Pristipomatidæ.

We print elsewhere a letter by Prof. Herdman on unusually large hailstones which fell at Liverpool on June 2. A correspondent writes to us from Rock Ferry about the same hailstones. "Five minutes after they fell," says our correspondent, "when the rain had subsided, I examined some. They were then all quite transparent outside, and had a spherical opaque centre surrounded by concentric rings alternately transparent and opaque. Some were flat: the largest I picked up measured one inch by three-quarters of an inch, and a quarter of an inch thick; its edge was very irregular. Others were more spherical in general form, but some still had rhombohedral faces very well defined. It was half an hour before all were melted."

On April 30, about 7 a.m., an earthquake was felt at Soken dal, in Dalerne, on the west coast of Norway. It was ac companied by a rumbling noise like distant thunder, going from east to west. The weather was clear and the wind north-west.

Serious efforts are being made to establish a great National Home-reading Circle Union, and we are glad to learn that they are likely to be successful. One object of the Union will be to publish courses of reading for three classes of readers—young persons, artisans, and general readers. The various courses will be approved by experts, and so planned as to interest and liberalize the mind as well as to convey useful information. Local circles will be formed under suitable leaders, and encouraged to meet frequently for the discussion of the subjects they have been studying. In many different ways the Union will try to aid the readers, and there can be little doubt that the scheme will be of real service to a very large number of persons who have often felt the need of guidance in their attempts at intellectual work. In all the courses science will receive the place which properly belongs to it, but adequate attention will also be given to literature and history.

Does the cuckoo ever hatch its own eggs? Herr Adolf Müller answers this question in the affirmative, and has given in the *Gartenlaube* a full account of a case which he himself claims to have observed. A translation of this account has appeared in the *Ibis*, and is reproduced in the new number of the *Zoologist*. The latter periodical prints also a translation of an article in which Herr Adolf Walter disputes the statements of Dr. Müller, who, he thinks, must have made a mistake. The same subject is dealt with in the June number of the *Selborne Magazine* by Mr. C. Roberts, who quotes from "Zoonomia" an interesting passage, in which Dr. Erasmus Darwin expresses his belief that the cuckoo sometimes makes a nest and hatches its own young. In this passage Dr. Darwin gives an extract from a letter of the Rev. Mr. Wilmot, of Morley, near Derby, describing an instance brought to Mr. Wilmot's notice in July 1792 by one of his labourers, and afterwards closely watched by Mr. Wilmot himself. Mr. Wilmot was confident that the bird was a cuckoo, but this is a point about which most ornithologists would no doubt like to have a little more evidence.

Everyone who takes the slightest interest in natural history will be sorry to learn that the kangaroo is in danger of being extinguished. Its skin is so valuable that large numbers of young kangaroos are killed, and high authorities are of opinion that, unless the process is stopped, Australians will soon have seen the last specimen of this interesting animal. Mr. R. G. Salomon, one of the largest tanners in the United States, whither kangaroo-skin is chiefly sent, urges that a fine should be imposed for the killing

of any kangaroo whose skin weighs less than ten-twelfths of a pound; and, from a note on the subject in the *Zoologist*, by Mr. A. F. Robin, of Adelaide, we are glad to see that a serious attempt is being made to secure the enforcement of this restriction throughout Australia and Tasmania, and the proclamation of a close season between January 1 and May 1. We must hope that the Australian Legislatures will understand the necessity of taking speedy action in this matter. It would be scandalous if, in deference to the wishes of a few greedy traders, they were to allow Australia to lose the most famous and most interesting of its characteristic fauna.

AT the concluding meeting of the thirty-first session of the Geological Society of Glasgow, which was held last week, Mr. John Young read a paper on "The Occurrence of Spines within Spines on the Shells of the Carboniferous Productidæ." He said that, though long familiar with the spines themselves, it was only recently he had discovered the peculiar structure he was about to describe. He pointed out that the spines of the Productidæ were tubular, and referred to the varied opinions expressed by those palæontologists who had previously taken up the group for investigation respecting the functions of these spines. Mr. Young said he had now discovered that within the tubes of the spines, of at least four species, there existed a barrier of small spines of microscopic size placed in the tubes just above the junction of the spines with the shell itself. These small spines were planted deeply within the walls of the large spines, and radiated to the centre of the tubes, varying from twenty to forty or more in number. He was inclined to believe that the detection of these minute spines favours the view held by De Verneuil, that the spines themselves conveyed water to the interior of the shell. In illustration of his paper, Mr. Young exhibited under the Society's microscope several beautifully prepared slides showing the presence and arrangement of these remarkable "spines within spines."

A SERIES of new aromatic compounds of bismuth have been prepared by Drs. Michaelis and Marquardt. The first member of the series, bismuth triphenyl, $Bi(C_6H_5)_3$, is a solid, crystallizing in two distinct forms of the monoclinic system, thus forming an example of dimorphism in the same system. In order to obtain it a quantity of the sodium-bismuth alloy is first prepared by melting 500 grammes of bismuth in a Hessian crucible and gradually adding 50 grammes of sodium in small pieces. The resulting hard alloy is then powdered in a warmed metallic mortar, and heated for two days to 160° C. in an oil bath with an equal weight of brom-benzene, C_6H_5Br, a little acetic ether being from time to time added. The filtered product is afterwards distilled until benzene ceases to pass over; water is then added, and the excess of brom-benzene distilled off in steam, the bismuth triphenyl remaining behind unaltered. The cooled residue in the distilling flask is next extracted with chloroform, the chloroform solution separated and dried over calcium chloride. After removal of the greater part of the chloroform by distillation, the bismuth triphenyl is thrown out of solution, by addition of absolute alcohol, as a brown oil. On standing the oil soon crystallizes, and on recrystallization from a mixture of alcohol and chloroform the bismuth triphenyl is obtained in beautiful long colourless monoclinic prisms of brilliant lustre. The prism angle of these crystals is 109° 40′, and the plane of the optic axes is perpendicular to the plane of symmetry. On the other hand, if pure warm alcohol be employed as the solvent, the crystals are found to be quite different, being tabular in habit, but still belonging to the monoclinic system. The principal prism angle is much less than that of the other form, being 100° 23′, and the plane of the optic axes is parallel to the plane of symmetry. No difference of composition could be detected, but the melting-point of the prismatic variety was found to be 78°, while that of the tabular form was 75°. Both varieties are converted by concentrated hydrochloric acid to benzene and bismuth trichloride—

$$Bi(C_6H_5)_3 + 3HCl = 3C_6H_6 + BiCl_3.$$

In a similar manner two higher homologues of the series, bismuth tritolyl, $Bi(C_6H_4 . CH_3)_3$, and bismuth trixylyl, $Bi[C_6H_3 . (CH_3)_2]_3$, were obtained by heating the bismuth sodium alloy with brom-toluene and brom-xylene respectively. Both form good crystals, but no dimorphism was observed. Perhaps the most important property of the compounds is their affinity for chlorine or bromine, for by direct addition they take up two atoms of either halogen, with production of compounds in which bismuth assumes its full pentad atomicity, thus supplying an additional proof of the similarity of bismuth to antimony, arsenic, phosphorus, and nitrogen. For instance, bismuth triphenyl forms $Bi(C_6H_5)_3Cl_2$ and $Bi(C_6H_5)_3Br_2$, the tolyl and xylyl compounds acting precisely analogously. Not only is this the case, but with nitric acid they form dinitrates of the type $Bi(C_6H_5)_3(NO_3)_2$, neutralizing two equivalents of the acid. It is interesting to note that the preparation of these aromatic compounds of bismuth completes a wider series, for the preparation of which we are mainly indebted to Dr. Michaelis and his assistants, as we are now acquainted with analogous compounds of antimony, arsenic, phosphorus, and nitrogen.

A VIPER and a lizard in spirit were lately sent, for examination, to the British Museum by Mr. R. H. Ramsbotham, Waterside, Todmorden, with the following remarks:—"This adder was caught at Trowbers Warren, Sussex, on April 24, 1889, about noon. It was kept in this bottle without spirit till the following morning, between nine and ten, when the bottle was filled. Immediately after this was done, the lizard (which is still in the bottle, and has not been touched) crawled out of the snake's mouth, and was quite lively for a short time." Commenting in the *Zoologist* on this statement, Mr. G. A. Boulenger points out that it includes three facts well worthy of record: (1) that vipers do occasionally swallow lizards, although their food normally consists of small rodents; (2) that in this instance the snake did not avail itself of its poison-apparatus in seizing its prey; (3) that a lizard retained life for nearly twenty-four hours in the gullet of a viper. The lizard is an adult female, *Lacerta vivipara*.

AMONG the contents of the new number of the Journal of the Anthropological Institute is an interesting paper, by Mr. T. W. Shore, on the distribution and density of the old British population of Hampshire. Miss Buckland has an instructive paper on the monument known as King Orry's grave, in the Isle of Man, compared with tumuli in Gloucestershire. There are also valuable papers on Australian message sticks and messengers, by Mr. A. W. Howitt; on social regulations in Melanesia, by the Rev. R. H. Codrington; and on the Nicobar islanders, by Mr. E. H. Man. The number contains Mr. Galton's Presidential address, which our readers have already had an opportunity of studying.

THE "Medical Register" and the "Dentists' Register," for 1889, have been issued. Both are published under the direction of the General Council of Medical Education and Registration of the United Kingdom.

THE Cambridge University Press has issued a second edition of Mr. M. M. Pattison Muir's "Treatise on the Principles of Chemistry." In the preface, Mr. Muir states that the whole has been thoroughly revised, and that Book II., dealing with chemical kinetics, has been entirely re-written.

MESSRS. LONGMANS AND CO. announce as nearly ready, "Physical and Chemical Studies in Rock-Metamorphism, based on the Thesis written for the D.Sc. Degree in the University of

London, 1888," by the Rev. A. Irving, D.Sc. Lond., Senior Science Master at Wellington College. The same publishers have in preparation seven new volumes of their "Elementary Science Manuals."

THE Smithsonian Institution has published a new edition of Mr. Frank Wigglesworth Clarke's "Tables of Specific Gravity for Solids and Liquids." It is, in effect, a new edition of Part I. of the work called "The Constants of Nature." The tables in this "part" have been revised, rearranged, and as nearly as possible brought up to date. The work is issued in England by Messrs. Macmillan and Co.

THE Abbé Armand David, whose writings on the natural history of China are well known in France, is contributing a series of articles to *Les Missions Çatholiques* of Lyons on the fauna of China. In the last issue of that journal he completed the papers on Carnivora.

THE *Publishers' Circular* states that a work entitled "The Ice Age of North America, and its Bearings on the Antiquity of Man," by Prof. G. F. Wright, is announced for early publication by Messrs. Appleton and Co., of New York. It will be amply illustrated from photographs taken by various members of the United States Geological Survey during the past ten years.

A WORK on the Island of Saghalin and its vertebrate fauna, by Dr. Nikolsky, has just been published. The author, a native of Astrakhan, is well acquainted with the fauna of the Altai and West Turkestan, and is known as the writer of an interesting work on the fauna of Lake Balkhash. In his new book he has utilized, besides his own collections, the very rich collections formed by the late M. Polakoff.

MR. W. P. COLLINS has issued a catalogue of works on *Cryptogamia*. He claims to possess a more complete set of books on Diatoms than have ever been advertised in a bookseller's catalogue.

WE have received vol. iii., Part 4 (second series), of the Proceedings of the Linnean Society of New South Wales. It contains the following papers:—Revision of the genus *Heteronyx*, with descriptions of new species, Part 1, by the Rev. T. Blackburn; Diptera of Australia—Part 4, the Simulidæ and Bibionidæ, by Frederick A. A. Skuse (Plate xxxix.); further notes on Australian Coleoptera, with descriptions of new genera and species, by the Rev. T. Blackburn; contributions towards a knowledge of the Coleoptera of Australia—No. 5, on certain species belonging to unrecorded genera, by A. Sidney Olliff; descriptions of hitherto undescribed Australian Lepidoptera (Rhopalocera), by W. H. Miskin; notes on Australian earthworms, Part 5, by J. J. Fletcher; descriptions of Australian Micro-Lepidoptera—Part 15, Œcophoridæ (continued), by E. Meyrick; on simple striated muscular fibres, by W. A. Haswell; jottings from the Biological Laboratory of Sydney University, by W. A. Haswell; Diptera of Australia—Part 5, the Culicidæ, by Frederick A. A. Skuse (Plate xl.); list of the Australian Palæichthyes, with notes on their synonymy and distribution, Part 1, by J. Douglas Ogilby; a list of the birds found in the county of Cumberland, N.S.W., by A. J. North.

THE additions to the Zoological Society's Gardens during the past week include a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. Walter Boden; a Serval (*Felis serval* ♂) from Zambesi, presented by Mr. John Walker; an Ocelet (*Felis pardalis* ♂) from America, a Red Brocket (*Cariacus rufus*), two White-eared Conures (*Conurus leucotis*) from Brazil, an Acouchy (*Dasyprocta acouchy*) from British Guiana, a Brazilian Tree-Porcupine (*Sphingurus prehensilis*), a Blue fronted Amazon (*Chrysotis æstiva*), a Yellow Hangnest (*Cassicus persicus*) from South America, a Hairy Armadillo (*Dasypus villosus*), a Pileated Jay (*Cyanocorax pileatus*) from La Plata, presented by Mrs. Wolfe; a Diana Monkey (*Cercopithecus diana*, var. *ignita* ♀), a Campbell's Monkey (*Cercopithecus campbelli* ♂) from West Africa, a Musanga Paradoxure (*Paradoxurus musanga*) from the Indian Archipelago, presented by Colonel Wethered; a Nightingale (*Daulias luscinia*), British, presented by Mr. John Young; a Bar-tailed Pheasant (*Phasianus reevesi*) from North China, presented by Mr. Charles J. Lucas; a Yellow-headed Conure (*Conurus jendaya*) from South-East Brazil, a Blue-and-Yellow Macaw (*Ara ararauna*), a Blue-fronted Amazon (*Chrysotis æstiva*) from South America, deposited; an American Jabiru (*Mycteria americana*) from Para, an American Tantalus (*Tantalus loculator*) from America, purchased.

OUR ASTRONOMICAL COLUMN.

NEW MINOR PLANET.—A new minor planet, No. 284, was discovered by M. Charlois, of the Nice Observatory, on May 29.

THE SPECTRUM OF χ CYGNI.—At the Wolsingham Observatory, bright lines were seen in the spectrum of χ Cygni on May 19 and 21; D_3 very plain. Confirmed by Mr. Taylor at Ealing.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JUNE* 9-15.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on June 9

Sun rises, 3h. 46m.; souths, 11h. 59m. 0·6s.; daily increase of southing, 11·6s.; sets, 20h. 12m.: right asc. on meridian, 5h. 11·2m.; decl. 22° 59′ N. Sidereal Time at Sunset, 13h. 26m.

Moon (Full on June 13, 14h.) rises, 14h. 48m.; souths, 20h. 35m.; sets, 2h. 9m.*: right asc. on meridian, 13h. 48·4m.; decl. 5° 45′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	4 52 ...	12 59 ...	21 6 ...	6 11·6 ...	22 6 N.
Venus.....	2 9 ...	9 14 ...	16 19 ...	2 26·1 ...	11 53 N.
Mars......	3 50 ...	12 9 ...	20 28 ...	5 21·3 ...	23 47 N.
Jupiter....	21 18*...	1 13 ...	5 8 ...	18 23·3 ...	23 9 S.
Saturn....	8 28 ...	16 3 ...	23 38 ...	9 15·5 ...	17 7 N.
Uranus...	14 23 ...	19 54 ...	1 25*...	13 7·3 ...	6 28 S.
Neptune..	3 4 ...	10 52 ..	18 40 ...	4 3·7 ...	19 8 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

June.	h.	
14 ...	9 ...	Jupiter in conjunction with and 0° 29′ south of the Moon.
14 ...	20 ...	Mercury at greatest distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
T Cassiopeiæ ...	0 17·2 ...	55 11 N. ...	June 9,		*M*
U Cephei	0 52·5 ...	81 17 N. ...	,, 9,	23 28	*m*
			,, 14,	23 8	*m*
R Geminorum ...	7 0·7 ...	22 53 N. ...	,, 14,		*M*
S Hydræ	8 47·8 ...	3 29 N. ...	,, 12,		*M*
U Hydræ	10 32·1 ...	12 48 S. ...	,, 9,		*m*
W Virginis	13 20·3 ...	2 48 S. ...	,, 10,	3 0	*m*
S Libræ	15 15·0 ...	19 59 S. ...	,, 14,		*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 12,	0 7	*m*
X Sagittarii... ...	17 40·6 ..	27 47 S. ...	,, 14,	1 0	*m*
Y Sagittarii... ...	18 14·9 ...	18 55 S. ...	,, 14,	2 0	*M*
R Sagittarii... ...	19 10·2 ...	19 30 S. ...	,, 13,		*M*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 13,	0 0	*M*
η Aquilæ	19 46·8 ...	0 43 N. ...	,, 13,	22 0	*M*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,, 12,	1 0	*m*
δ Cephei	22 25·1 ...	57 51 N. ...	,, 10,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	250°	... 20° S.	
Near ζ Cygni...	318	... 30 N.	
,, β Piscium	345	... 0	... Very swift.

NOTES ON METEORITES.[1]

IX.

Did these Swarms or Comets always belong to the System?

Must we assume that the members of the swarms to which we have referred and of all the other swarms similar to it have always been thus crossing the earth's orbit periodically; that the November swarm, to take an instance, has *always* been crossing it every thirty-three years? Must they of necessity have started their existence with the planets and other more stable members of the system?

This point has been well inquired into, and it is certain that it is not at all necessary that such a state of things should have existed from all time.

It is a matter of common knowledge that all stars are in motion. The so-called "fixed" stars are not really fixed: they are only relatively fixed. The sun is a star, and therefore like the other stars it is also in movement with its attendant bodies in space.

If we have a swarm of meteorites moving in space, as the sun is doing, at a very considerable distance from the sun, the directions of movement being not parallel but inclined to each other, a time will come when the two bodies, taking the swarm as representing one body, and the sun another, will begin to have an attractive influence on each other. If the attractive energy of the sun is considerable as compared with that of the swarm, the swarm will begin to change its direction obviously towards the sun. If, in changing its direction towards the sun and increasing its velocity in consequence of this increased gravitational stress, that swarm can get round the sun without any loss of momentum the two bodies will say good-bye to each other and will go different ways; but supposing there has been a loss of momentum the loss may mean that for the future the swarm of meteorites must perform its journey *round the sun*.

It does not therefore follow that when a particular group of meteorites has been watched for 900 years that these meteorites which give rise to the appearance of shooting stars always formed part of the solar system. What we do know is that *at the present moment* this particular swarm to which the November meteors are due and another swarm which is called the Biela swarm, to mention two instances, do really move round the sun in closed cometary orbits, and the chronicle of the appearances of both these swarms is so complete that very definite statements may be made about them.

With regard to the November swarm it is known that a thousand millions of miles of its orbit have been pierced by the earth in its successive passages through it since the year 902, each time the earth must have filched many millions of the small constituents of the swarm and used them up as shooting stars, and yet the swarm does not seem to be very much the worse, and enormous though the numbers are, it is known that the distances between the meteorites is so considerable that no obvious mutual gravitational effect can be noted, so that their combined or common movement is a clear indication of a common origin.

In the case of the orbit of the Biela swarm we know that more than half of it, or a length of 500 million miles, contains these meteorites; a long thin line, say a mile long and an inch in section, represents, according to Prof. Newton, the distribution of the meteorites along the orbit.

The great Laplace was the first to suggest that many comets, especially those of high inclination and great eccentricities, represented introductions of matter into the solar system from external space. But on this, as on many other points, we owe our present views chiefly to Schiaparelli, who, in 1867, attacked the problem[1] in connection with his researches on the November swarm.

He commenced by referring to the point made by Laplace as to the phenomena presented by cometary orbits, suggesting that the planets are truly indigenous to the system; have always followed the sun in his movement through space; and had taken part in all the evolutionary changes which have finally brought the solar system to its present condition. In these characters common to planets the comets are lacking, while the eccentricities of their orbits generally is so great that the greater part of their journey is performed outside the known limits of our system. Schiaparelli considers that these facts demonstrate that the comets were not members of the solar system during its early stages, but that they are really messengers from the stellar void. Cloudlike masses wandering in parts of space where there was no star sufficient to dominate them have fallen gradually under the empire of our own by the effect of their movement relatively to our system. This movement, combined with the acceleration produced by the large mass of the sun has determined the relative orbits of these bodies in relation to the sun, which is very different from their absolute orbit in space. He next examined all the circumstances of the movement of the external mass under these conditions. First, there is no doubt that the movement of the solar system in space is comparable to that of the planets in their respective orbits, while it is possible—indeed certain—that many of the stars are in more rapid movement than our sun. Hence when it is affirmed that the relative movement of the sun and of other bodies disseminated through space is comparable in rapidity to the orbital movement of the planets, the statement is not a surprising one.

That being so, let us next suppose that one of these cloudlike masses—let us call them external swarms—wandering in space in consequence of its initial movement, penetrates eventually into a region where the attraction of the sun is much greater than that of any other star. It might be situated at a very great distance from the sun, where the annual parallax is only a small number of seconds. The relative movement will take place in a conic section. To define it, let us suppose the sun stopped, and let us give to the comet, instead of its real velocity in space, its relative velocity to the sun; and let us further imagine a perpendicular dropped from the sun in the direction of this relative velocity. It is evident that the area described by the comet round the sun in unit time will be equal to the half of the product of this perpendicular by its relative velocity.

Now as in general this velocity is of the order of planetary velocities, and since most frequently the perpendicular in question will be very much greater than the distance of the planet from the sun, we must conclude that the areas described by the comet round the sun in unit time will be incomparably greater than the corresponding areas described by the planets. But when many bodies move in conic sections round a central body, the areas described in unit time are, among themselves, as the square roots of the parameters of their respective orbits; therefore, the ratio of the parameters of cometary orbits to those of planetary orbits will be much greater than the ratio of the areas described by comets to the areas described by planets in unit time. Whence it follows that, in general, cometary orbits will have enormous dimensions in every direction, and that bodies which describe them will remain perpetually invisible to us, in consequence of their enormous distance. Nevertheless, among the infinite combinations possible in cometary orbits, there are two which may bring the cometary cloud within our ken: one, when the comet is moving directly towards the sun, describing a hyperbolic orbit very little different from a right line; and the other, when the relative movement of the comet and sun is almost zero, that is, when the two bodies are moving through the stellar space along parallel lines with nearly equal velocities.

Schiaparelli then goes on to show that when these cosmic clouds are attracted by the centre of our system, the constituent particles of the cloud must be drawn out into a parabolic current; thus, for instance, supposing a cosmic cloud equal in volume to the sun and at such a distance that its apparent diameter is 1′, the sun's attraction upon this would result in the formation of a parabolic chain or stream of such a length that it would require 636 years to pass through perihelion. When the centre was close to the sun, the beginning and the end of it would be distant from it 263 times the earth's distance from the

[1] Continued from vol. xxxix. p. 402.

[1] *Les Mondes*, vol. xiii. p. 147.

sun. There are nebulæ of which the apparent diameter is greater than that of the sun. If we assume such a nebula, with the sun's apparent diameter, 1924", it would be transformed into a parabolic chain which would require 20,000 years to pass perihelion, its transversal dimension still being such that the earth could traverse it in one or two days at the most. In this way, then, Schiaparelli shows not only that external swarms can be attracted from external space into our system, but that when so drawn out their constituent particles must take the form which we know such swarms as that of November to possess.

More recently this subject has been treated by Prof. H. A. Newton, and some results at which he arrived have been thus stated by Prof. A. S. Herschel:—[1]

"The evidence so strongly and distinctly shown in favour of the theory of the original motion of most, if not of all, of our recorded comets in spaces far external to the solar nebula, rests upon the assumption that the comet-yielding matter of the primitive nebula, if it existed, was confined, like that which formed the planets, to the neighbourhood of the ecliptic plane. This ground for the conclusion may admit of an exception that a similar distribution of the inclinations of the orbits to that which Laplace's hypothesis requires, would have been produced were this matter otherwise spread uniformly on a very distant sphere, instead of in the distant portions of a disk or annulus. But the plane of the planetary motions in the solar system, and the analogy which they present to spiral and disk-like nebulæ in the heavens, scarcely allows us to assume with reasonable probability such a different disposition of the matter of the outer part of the nebula from what the courses of the planets show us must have been its original mode of distribution and of gradual contraction near the centre; and with no evidence before us of the past or present existence of a distant spherical envelope of nebular matter inclosing the solar system, we may certainly prefer to accept, with Prof. Newton, the much simpler conclusion to which he is finally conducted by his well-executed labours, that, with the exception of a few, perhaps, of the zodiacal comets, and comets of the shortest periods, all the comets which have been recorded are originally denizens of the interstellar spaces, pursuing unknown orbits like the stars, and separated at least and dissevered in their primitive astronomical relations from any connection with the nebular matter which, in the process of concentration supposed by the nebular hypothesis, formed the sun, the planets, and the asteroids."

MOST COMETS HAVE ONCE EXISTED AS EXTERNAL NEBULÆ.

We now come to an important question: we have noted the extreme probability that the comets which now form part of the solar system did not always belong to it, but that they were drawn into it by the sun's attractive energy in its course through regions of space which contained the meteorites of which they are composed.

Then, instead of considering the case of a cloud of meteorites at a great distance from the sun, we have to consider one moving in an orbit round it; and we must attempt to inquire into the conditions of that cloud, both before and after it began to fall under the sun's attraction. Into what conditions must we inquire in order to compare comets with external swarms? They are mainly these—

(1) It is agreed that a comet is a swarm of meteorites, each meteorite being on an average far from its neighbours. This follows from an inquiry into the masses of comets. These are very small, for they have never been known to appreciably disturb any of the planets, or even the satellites, by their gravitational attraction.

In 1776, Jupiter and his satellites were entangled in a comet, yet the satellites pursued their courses as if the comet had no existence. The comet itself, however, was thrown entirely out of its course by the gravitational influence of the enormous mass of Jupiter, and its time of revolution changed from a long period to a short one of twenty years or so.

Biela's comet, first seen in 1826, appeared as a double comet in 1845. The extreme lightness of the two portions was shown by the fact that their mutual attraction was imperceptible, and each performed its revolution independently of the other.

The mass of a comet probably never exceeds 1/5000 of that of our globe. The meteorites composing them must therefore be very far apart, seeing that this small mass is distributed through spaces millions of miles in extent.

[1] *Monthly Notices*, vol. xxxix. p. 279.

(2) We next assume that a comet's luminosity is to a large extent produced by collisions of meteorites.

It is certain that one of the principal causes of the increase of temperature of a comet during its approach to perihelion is the increased number of collisions due to the greater tidal action which takes place. Hence the larger the swarm, the greater the difference between the attractions of the sun upon opposite sides of it, and therefore the greater the disturbance set up. Also, the shorter the perihelion distance, the greater fraction of it is the diameter of the swarm, and the greater therefore the differential attraction.

The initial movements of the individual members of the swarm, and these superadded by tidal action, may be defined as producing *internal* work.

If all the heat of a comet is produced by such internal work, it is clear that the temperature of the comet will depend (1) upon the velocity of orbital motion of the particles, (2) upon the size of the swarm of which it is composed, and (3) upon its perihelion distance. It will practically be independent of the velocity of the comet in its orbit round the sun.

If the luminosity be due entirely to internal collisions brought about by the increase of solar action, then large comets, or those best visible, should begin to be brilliant long before smaller or more distant ones. But this does not seem to be so. Mr. Hind has pointed out that proximity to the earth is not so important a condition for visibility of a comet in the daytime as close approach to the sun (NATURE, vol. x. p. 286); and M. Faye is the authority for the statement that no comet has been seen beyond the orbit of Jupiter (*ibid.* p. 228). "It is assuredly not on account of their smallness that they thus escape our notice in regions where the most distant planets, Saturn, Uranus, and Neptune, shine so clearly with the light which they borrow from the sun; this is because the rare and nebulous matter of comets reflects much less light than the solid and compact surfaces of the planets of which we speak, much less even than the smallest cloud of our atmosphere."

On the latter part of this quotation it may be remarked that it is not necessary to assume that comets at a great distance from the sun, any more than nebulæ, are visible by means of reflected light.

Another possible cause is that of collisions with bodies external to the comet. If external work is done on a comet by meteorites in space—that is to say, if there are collisions with external bodies—the velocity of the comet must be considered in the first place, and the equal or unequal distribution of the masses which it encounters can be tested by the phenomena observed.

The discussion of the recorded observations shows, indeed, that in addition to the constantly increasing action which takes place in a comet during its approach to perihelion passage, there are at times temporary increases in temperature.

We know that meteorites are scattered through space, and here and there are gathered into swarms. It is only to be expected, therefore, that at times a comet will meet with such swarms just as our own planet does, and in that case its temperature would be increased by the collisions which would occur. The increase of temperature would depend upon (1) the dimensions and density of the swarm; and (2) upon its velocity. The larger and denser the swarm the more collisions would be likely to occur, and the greater the velocity of the comet, the greater the amount of kinetic energy available for transformation into heat energy.

If we assume that the increased brightness of comets as the sun is approached depends to any extent on collisions with meteorites external to the swarm, we must conclude that such meteorites exist nearer together nearer the sun. This we should expect. A test of this view would be great and irregular variations of intensity, as we know that the meteorites which the comet is liable to meet are not equally distributed. Such a variation was noticed in Sawerthal's comet in 1888, amounting to three magnitudes (NATURE, vol. xxxviii. p. 258) in two days.

Such variations, however, would be more likely to be observed in the tails in consequence of the enormous dimensions of some of them; and indeed they have been observed from the time of Kepler.

The fact that these variations so strongly resemble at times auroral displays is an additional argument in favour of the meteoric origin of the latter.

Another result of a different order produced by a comet moving through a meteoric plenum would be the gradual shortening of a comet's periodic time, and this shortening should not be abso-

lutely regular, as in a homogeneous gas, for the reason that the meteorites are not equally distributed.

That there is such a shortening was proved by Encke for the comet which bears his name, as the following table will show :—

Returns of Encke's Comet, showing Reduced Period of Revolution.

	Observed Period of Revolution. days.	h.	m.	Difference. h.	m.
From 1786 to 1795 three times.........	1212	15	7		
,, 1795 ,, 1805 ,, ,, ...	1212	12	0	3	7
,, 1805 ,, 1819 four ,, ...	1212	0	29	11	31
,, 1819 ,, 1822	1211	15	50	8	39
,, 1822 ,, 1825	1211	13	12	2	38
,, 1825 ,, 1829	1211	10	34	2	38
,, 1829 ,, 1832	1211	7	41	2	53
,, 1832 ,, 1835	1211	5	17	2	24
,, 1835 ,, 1838	1211	2	38	2	39
,, 1838 ,, 1842	1210	23	31	3	7
,, 1842 ,, 1845	1210	21	7	2	24
,, 1845 ,, 1848	1210	18	29	2	38
,, 1848 ,, 1852	1210	17	2	1	27
,, 1852 ,, 1855	1210	11	17	5	45
,, 1855 ,, 1858	1209	13	41	21	36

Here, then, we have three possible sources of collisions. In any case, if any light be produced by collisions, we have the spectroscope as a sure guide to enable us to determine its chemical origin.

We have already seen that the telescopic appearance of a comet when far away from the sun and when close to it are very different. We must now introduce the verdict of the spectroscope. It was observed by Dr. Huggins in the comets of 1866 and 1867 that when they were very far away from the sun the spectrum consisted chiefly of a line seen in the spectrum of those nebulæ which he had up to that time examined. Unless, then, Dr. Huggins has withdrawn this observation, there is a *spectroscopic* connection between nebulæ and comets away from the sun.

The phenomena of comets revealed by the telescope show, as we have seen, that as a matter of fact a good many of them seem to be connected in some way or other with the production of luminous concentric or eccentric envelopes.

In the case of a comet gradually getting nearer the sun, and getting very excited as it gets there, we pass from the spectrum already described to a very different one. There is a considerable change similar to that observed in experiments with meteorites, the spectrum of carbon produced from some compound of carbon or another. In nineteen cases out of twenty when the comet gets near the sun and near enough to the earth for us to have a good look at it, the spectrum is a spectrum of carbon.

On its first appearance in a cometary spectrum, carbon is represented by the flutings which are special to low temperatures. In the most visible part of the spectrum these flutings differ very little in position from those which appear at a higher temperature, but in the blue there is a low-temperature fluting about wave-length 483, whilst the nearest high-temperature fluting is 474. If, therefore, this fluting be observed, the presence of cool carbon may be safely inferred, although it would not be quite safe to infer its presence from observations of the green flutings. This has certainly been observed in two comets—namely, Winnecke's comet (1868) on June 17 (the perihelion passage occurring on June 24), and in Brorsen's comet (1879) on March 25 (the perihelion passage occurring on March 30). The limited number of recorded appearances of cool carbon in comets is doubtless due to the same cause as in the case of the line near λ 500, which Dr. Huggins ascribed to an unknown form of nitrogen, while I ascribe it to magnesium, since we know that there is magnesium in meteorites, and we do not know that there is an unknown form of nitrogen. The reason is that the temperature being low, the light is excessively feeble and observations therefore difficult. When nearer perihelion passage, the comets get hotter, and the spectrum of cool carbon is replaced by that of hot carbon. Under these conditions of increased temperature, comets lend themselves best to spectroscopic study, and hence it happens that in the majority of cases the spectrum of a comet (if the temperature be increasing) has not been observed until it has arrived at this stage.

Manganese is the next substance which writes its record in the spectroscope. It is first represented by a fluting at 558,[1] which is the brightest fluting in its spectrum at a low temperature. This fluting is very persistent, and becomes visible even when there is only a very small percentage of manganese present in the substance examined. The fluting is always seen before the iron lines in the spectrum of ordinary iron at the temperature of the oxyhydrogen flame, and this is the case even with the purest specimens of electrolytic iron which has yet been prepared. The effect of the addition of this fluting to the spectrum of carbon is to modify the appearance of the citron band in the cometary spectrum in a very definite manner.

At a still higher temperature, the radiation of lead is added to that of manganese and carbon, which still further modifies the appearance of the citron band. The brightest lead fluting is at wave-length 546, and when this is present in the spectrum of a comet the citron band has three maxima of brightness, one at 564 (carbon), one at 558 (manganese), and one at 546 (lead).

Afterwards, the temperature having increased, the radiations of manganese and lead give way to the absorption flutings of these substances, carbon radiation from the interspaces still remaining. The result is again a very definite modification of the appearance of the citron cometary band, the general effect being an apparent shifting of the carbon fluting from wave-length 564 to a more refrangible part of the spectrum—namely, to 558, when only manganese absorption is added, and to 546 when both manganese and lead absorptions are added.

Until quite recently, the variations in the position of the citron band in different comets, or in the same comet at different periods, have been attributed to faulty observations, it being supposed that carbon pure and simple was in question. It is now certain, however, that this is not so in all cases. The variations are real, and are simply dependent upon the temperature, or indirectly upon the distance from perihelion.

In some cases iron fluting absorption has also been observed under these conditions of high temperature.

The spectral conditions brought about in the comets which in our time have got nearest to the sun were very similar to those observed in the electric arc, and the recorded observations of the spectrum show that we were dealing with a considerable number of lines of iron, manganese, and other substances.

We see in the telescope that a comet puts on the appearance of a central nucleus with surrounding envelopes or jets, so that we must understand that in the spectroscope the spectrum of the nucleus is seen distinct from the spectrum of the envelopes and jets, because the former is made to fall upon one part of the slit of the spectroscope and the latter upon another.

When a comet approaches very near to the sun we get in addition to the usual flutings of carbon, bright lines, *especially in the spectrum of the nucleus*, so that in addition to the long flutings of carbon as visible in the spectroscope we have short lines added along the nucleus in the red, yellow, green, and so on.

In those comets which have reached a very high temperature, like Comet Wells and the Great Comet of 1882, there is evidence of line absorption. At the same time there were bright lines, proceeding from the incandescent vapours driven away from the meteoritic nuclei by the solar repulsion. Without this repulsion, it is highly probable that there would be line absorption pure and simple, and this has to be taken into account in comparing the spectra of comets with the spectra of other meteor-swarms.

During the passage of a comet from perihelion to aphelion, the temperature decreasing, these changes take place in inverse order.

In spectral phenomena, then, we have another term of comparison to apply, and it may be stated that the sequence of spectral changes are now known to us in a very definite way, so that the chemical changes which take place in the composition of the vapours of comets produced by collisions at various distances from the sun have been ascertained.

Whether we take form, distance apart of component parts, or spectrum, there is now ample proof that the external bodies which supply us with these shreds and patches which we term comets are the nebulæ.

It has already been stated that if we can rely upon Dr. Huggins's observations, in some comets at aphelion and in some planetary nebulæ we get a single line at the same wave-length, so that from

[1] Students of spectrum analysis will understand that this is a "short title," and does not represent the *exact* wave-length, which with adequate instruments might require something between 10 and 100 numerals.

this observation alone it would seem extremely probable that when a comet enters our system for the first time it simply means that a swarm of meteorites in that part of space through which the sun was passing at the time began to feel the sun's attraction, and ultimately became a member of our system, and also that when we see the appearance which we call a nebula in space, since its spectrum is the same as the spectrum of a comet, the nebula is simply a swarm of meteorites if it be true that a comet is a swarm of meteorites.

These nebulous masses, visible in all parts of the heavens, but in some parts of the heavens very much more numerously than in others, were very early observed and imagined to be very different in nature from the so-called fixed stars.

Ptolemy was the first to point out, when he was making his map of the stars that there were certain "cloudy" stars of which he gave 5 on his map, and Tycho Brahé, whose work was done before the invention of the telescope, although he did not notice any bodies which we now class as nebulæ, was firmly convinced that that nebulous luminosity, which we call the Milky Way, was something entirely different in its nature from the stars. He imagined it to be what he called an ethereal essence, a sort of fire mist, so that when in his time, in the year 1572, a new star appeared, he supposed it to be a considerable agglomeration of this ethereal fluid. Galileo was able to show that the Milky Way, the "ethereal substance" of Tycho, was only an appearance due to enormous numbers of stars lying in the same visual ray, the stars of which the Milky Way is composed can indeed be seen with very small optical power. It was not till 1612, a few years after the introduction of the telescope, that we got the first real definition of a body which we now call a nebula.

The first observation we owe to Simon Marius, who stated that some of the bodies visible exactly resembled the appearance produced by the flame of a candle seen through horn. It was not till 1656 that the nebula in Orion was discovered, although now to the trained eye it is very easily visible, so that it seems rather wonderful that it was not discovered before. In 1714, in England, attention began to by paid to these bodies, but it was not until the time of Sir Wm. Herschel that the most magnificent revelations were made. He was the first to construct very large telescopes, the function of very large telescopes being to collect light, so that objects which appear to the eye as excessively dim may be brought into full visibility.

After not only Sir Wm. Herschel but his son, Sir John Herschel, had accumulated vast stores of facts, Lord Rosse took up the story, and made a telescope very much more powerful than any which had been employed by the Herschels. His telescope has a light-grasping power compared with the eye of 130,000. One of the results of Lord Rosse's work to which we need here refer is the idea that in a great many bodies which had been classed as nebulæ this enormous increase of optical power suggested that we were only dealing with very distant clusters of stars.

Lord Rosse was able to get the suggestion of "resolvability" in so many bodies which had been classed as nebulæ by Sir Wm. Herschel and others, that gradually the idea came to be held that the most nebulous nebula, if we could get sufficient optical power to bear upon it, would be broken up into stars, just as certainly as the Milky Way had been.

This would mean that the nebulæ were simply clusters of stars so infinitely remote from our ken that even with the power of Lord Rosse's instrument they put on the appearance of an ethereal essence.

This was the general opinion in 1864, in the early days of spectrum analysis, when Dr. Huggins turned his spectroscope one night to one of the planetary nebulæ. At first he thought that something had gone wrong in the apparatus because he could only see a bright line instead of the usual sort of spectrum obtained from a star. The spectroscope, however, was doing its level best, and the cause, the anomaly, was really that the nebula gave out monochromatic light.

In some cases another line was seen, stated to be due to hydrogen. It now appears that the dispersion employed was so small that the discoverer had no right to allocate any line, so that it is fortunate that other observers have since shown that there is another hydrogen line visible.

Dr. Huggins came to the conclusion that the first line was very nearly, if not exactly, in the position of the chief line seen in the spectrum of nitrogen, and the suggestion was therefore made that these nebulæ were masses of nitrogen and hydrogen gases mixed, or, if not nitrogen, some constituent of nitrogen mixed with hydrogen. That result made the idea of Lord Rosse concerning the possibility of the resolvability of nebulæ into stars untenable. We had to consider from that time that the light of the nebulæ came from a gas, and hence it was held that the nebulæ were masses of gas.

Another explanation of the origin of the green line has already been given. If we study the spectrum of magnesium we find a very bright fluting with its less refrangible edge absolutely in the position of the green line with the dispersion generally employed; in nebulæ and in comets the same line appears, if, as I said before, Dr. Huggins's observations are to be relied upon.

We are therefore justified in holding the view that nebulæ, like the comets, consist of meteorites.

J. NORMAN LOCKYER.

(*To be continued.*)

THE ANNUAL VISITATION OF THE GREENWICH OBSERVATORY.

THE Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich, was received at the annual visitation on Saturday last, June 1.

As regards buildings, it is noted that the new 18-foot dome is completed, together with the photographic dark rooms, in preparation for work with the 13-inch photographic equatorial which is to be erected this year. As regards transit-circle observations, we read:—

"The regular subjects of observation with the transit-circle are the sun, moon, planets, and fundamental stars, with other stars from a working catalogue, which includes all the stars in Groombridge's Catalogue and in the Harvard Photometry not observed since 1867, and a selection from Piazzi's Catalogue. Ten close circumpolar stars taken from the *Connaissance des Temps*, or from M. Lœwy's list of stars for longitude determinations, have been observed regularly, in addition to the four standard azimuth stars. The observation of these close circumpolars has been much facilitated by the adoption (since 1889 January 1) of the method used by the officers of the French Service Géographique, which consists in making a number of bisections of the star with the R.A. micrometer during its transit, the exact time for each bisection being recorded on the chronograph. The Annual Catalogue of stars observed in 1888 contains about 1820 stars.

"Special attention has been given to the observation of the minor planet Iris and comparison stars in connection with the determination of its parallax at the late favourable opposition, eighteen observations of the planet and 113 of twenty-eight comparison stars having been made last autumn."

As regards computations, the transits have been completely reduced so as to exhibit mean Right Ascension 1889 January 1, and also the circle observations to exhibit mean North Polar distance for the same period. Two determinations of the astronomical flexure of the transit-circle telescope have been made since the last Report, the resulting values being $0''\cdot08$ and $0''\cdot52$. It has been found that the correction for discordance between reflection and direct observations of stars was erroneously applied in 1887, and hence the results for colatitude and for position of the ecliptic are also erroneous. The correct values are now given, with those recently found for 1888.

The ecliptic investigations from 1877 to 1886 have been revised to reduce the results to the same system of flexure, R—D correction, refraction and colatitude; so the computations for the ten-year Catalogue, containing 40,000 observations of 4059 stars, are now practically complete.

It has been found that the mean error of the moon's tabular place (computed from Hansen's lunar tables with Newcomb's corrections) is $+\,0^s\cdot090$ in R.A., and $+\,1''\cdot21$ in longitude, as deduced from seventy-four meridian observations in 1888. The mean error in tabular N.P.D. is $-\,1''\cdot19$, indicating that the mean of the observed N.P.D.'s is too great. A number of altazimuth observations has been made and reduced to April 8, so as to exhibit errors of moon's tabular R.A., N.P.D., longitude, and E.N.P.D.

The object-glass for the new 28-inch refractor is now being worked, and, as it is to be of a special form, equally suitable for photographic and eye observations, an experimental object-glass

is being mounted on the Sheepshanks equatorial for trial. A number of photographs of stars have been taken with the experimental 6-inch object-glass, supplied as a preliminary to the construction of the 13-inch, which is to take part in the construction of the photographic map of the heavens. Only inconclusive results have, however, as yet been obtained. The spectroscopic work has mainly consisted of observations of motion of stars in line of sight. We read:—

"For determination of motions of approach or recession of stars, 236 measures have been made of the displacement of the F line in the spectra of 38 stars, and 18 of the *b* line in the spectra of 8 stars, besides 5 of the *b* line in the spectrum of Saturn's ring, and comparisons with the spectra of the moon, Venus, the sun, or the sky, as a check on the general accuracy of the results. Observations of Algol on three nights during the past year confirm the previous results indicating orbital motion, but further evidence is required to establish the fact. The spectra of γ Cassiopeiæ, o Ceti, β Lyræ, P Cygni, R Cygni, and β Pegasi, have been examined on several occasions, and Comet e 1888 has been spectroscopically observed on one night, the spectrum being chiefly continuous. The spectroscopic observations of all kinds are completely reduced."

Photographs of the sun have been taken 182 days in the year ending on May 10, 1889. Indian and Mauritius sun photographs have been received from the Solar Physics Committee as far as 1888 December 31 and December 9 respectively, and it is noted that, by means of photographs from these two places supplementing the Greenwich series, the daily photographic record of the sun's surface is practically complete since the beginning of 1882. For earlier years 118 photographs of the sun taken at Harvard College, Cambridge, U.S.A., between 1874 December 9 and 1875 December 31, and ten photographs taken at Ely, between January 1 and February 25, 1874, have been received from the Solar Physics Committee.

The photographs of the sun for 1888 show that it has been free from spots on 155 days in the year 1888, the longest spotless periods being February 4 to 17, May 24 to June 8, and October 5 to 25. The mean spotted area in 1888 was half that of the preceding year, and corresponded closely to that for 1877, so that the minimum may be expected to occur during the present year. The mean distance of spots from the equator has also diminished to 7°·38 in 1888, being very little larger than it was in 1878, just before the last minimum, and this is a further indication that the sun-spot minimum is close at hand. The faculæ in 1888 show a diminution in correspondence with that of sun-spots, their area for 1888 being intermediate between those for 1876 and 1877.

Continuous observations of the changes in the three magnetic elements of declination, horizontal force, and vertical force have been photographically recorded.

The following are the principal results for the magnetic elements for 1888:—

Approximate mean declination	17° 40′ W.
Mean horizontal force ...	3·9480 (in British units).
	1·8204 (in Metric units).
Mean dip	67° 24′ 26″ (by 9-inch needles).
	67° 25′ 33″ (by 6-inch needles).
	67° 26′ 16″ (by 3-inch needles).

In the year 1888 there were only three days of great magnetic disturbance, but there were also about twenty other days of lesser disturbance, for which tracings of the photographic curves will be published, as well as tracings of the registers on four typical quiet days.

The meteorological results are as follows:—

"The mean temperature of the year 1888 was 47°·7, being 1°·6 below the average of the preceding forty-seven years. The highest air temperature in the shade was 87°·7 on August 10, and the lowest 18°·4 on February 2. The mean monthly temperature in 1888 was below the average in all months excepting May, November, and December. In March, April, July, and October it was below the average by 3°·6, 3°·6, 4°·4, and 3°·9 respectively, and in November it was 4°·0 above the average.

"The mean daily motion of the air in 1888 was 296 miles, being 12 miles above the average of the preceding twenty-one years. The greatest daily motion was 790 miles on March 11, and the least 57 miles on December 31. The only recorded pressures exceeding 20 lbs. on the square foot were 31 lbs. on March 11, and 21 lbs. on August 28.

"During the year 1888 Osler's anemometer showed an excess of about nineteen revolutions of the vane in the positive direction north, east, south, west, north, excluding the turnings which are evidently accidental.

"The number of hours of bright sunshine recorded during 1888 by the Campbell-Stokes sunshine instrument was 1068, which is about 250 hours below the average of the preceding eleven years, after making allowance for difference of the indications with the Campbell and Campbell-Stokes instruments respectively, and it is 333 hours below that of 1887 recorded with the same instrument. The aggregate number of hours during which the sun was above the horizon was 4465, so that the mean proportion of sunshine for the year was 0·239, constant sunshine being represented by 1. A comparison has been made of the records of the Campbell and Campbell-Stokes instruments for the twelve months from 1886 June 1 to 1887 May 31, with the result that the former registered 1256 hours of bright sunshine, while the latter registered 1364. It would appear, therefore, that the indications of the former instrument require to be multiplied by the factor 1·086 to make them comparable with those of the latter.

"The rainfall in 1888 was 27·5 inches, being 2·9 inches above the average of the preceding forty-seven years."

The average daily number of chronometers and deck watches being rated is 212, and the total number received up to May 10, 1889, was 668. The Astronomer-Royal notes that in future the duration of the trial of deck watches will be increased from twelve to sixteen weeks, viz. six weeks in the ordinary temperature of the room, four weeks in the oven (temperature 80° to 85°), and finally six weeks in the room.

The Report concluded with a note on the re-determination of the difference of longitude between Greenwich and Paris:—

"Observations were made in four groups of three nights each (or the equivalent in half nights). An English and a French observer were stationed at each end, each with a separate instrument and chronograph, and the pairs of observers were interchanged twice, to eliminate any change in the personal equations during the progress of the work. The pairs of English and French instruments were similar, and the signals as well as the star transits were recorded on similar chronographs. On a full night each observer recorded about forty star transits, reversing his instrument three times, and exchanged signals twice (near the beginning and end of the evening) with his compatriot at the other end of the line, and once with the other observer. At Greenwich the transits were referred to the sidereal standard clock, and comparisons with the large Greenwich chronograph enable the ordinary determinations of clock-error with the transit-circle to be utilized as well as those specially made with the portable transits. With this object transits of clock stars with the transit-circle were usually taken by four observers on each night during the longitude operations. The actual stations were the Front Court of the Royal Observatory and the Observatory of the Service Géographique de l'Armée at Paris, the position of which reference to the Paris Observatory has been accurately determined. Commandants Bassot and Defforges were the French observers, and Mr. Turner and Mr. Lewis the English. The observations lasted from September 23 to November 15, and 18 nights of observation at both stations are available, the two English observers having observed at Greenwich 653 transits of clock stars and 165 of azimuth stars, and at Paris 778 transits of clock stars and 165 of azimuth stars. All of these, as well as the signals exchanged, have been read out from the chronograph registers and the reductions are far advanced. Subsidiary investigations of the value of a revolution of micrometer screw, of intervals of wires, of form of pivots, and of errors of the axis-level have consumed much time, the last-named having been a long and tedious discussion."

The difference in longitude between Greenwich and Dunkerque will be determined this month, and Commandant Defforges also proposes to determine the latitude between these two places.

THE EARTHQUAKE.

On the evening of Thursday, May 30, a considerable seismic disturbance was noticed over the English Channel and in the neighbouring districts. Its area cannot yet be precisely determined. It seems to have been felt most strongly in the Channel Islands, but it was also very distinctly noticed over wide districts in the south of England and the north of France. We bring together various facts relating to the earthquake, some of which have been communicated to us by correspondents.

In Guernsey four successive vibrations were felt at 8.15 p.m. It is said that the houses in St. Peter-Port trembled for several seconds, and that most of the occupants rushed into the streets. The weather had been very sultry for some hours previously.

The shock was felt not less severely in Jersey, as the two following letters will show. The first, which has been sent to us from the Meteorological Office, is by the correspondent of that Office at Jersey, and is dated St. Aubin's, May 31:—"Last evening, at 8.15 p.m., there was a rather severe shock of earthquake here; in fact, it visited the greater part of the island, as far as I can learn. But what I heard myself was a loud rumbling noise, and everything began to shake and tremble, even the buildings; it so frightened a great many that they ran out of their houses, not knowing what was the matter. It appeared to me to travel from north to south, and lasted for about two to three minutes. The barometer at the time was steady, the wind south-south-west, force 4, and a fine night with a clear starlit sky; this morning the weather was gloomy. There was a mock sun, and afterwards a solar halo, but it has been a fine day, with a deal of cirro clouds moving from a southerly direction and looking very heavy in the westward all day, and at present we have cold nights." The second letter has been sent to us by the Rev. W. Clement Ley, to whom it was written, on May 30, by a friend at St. Helier's, Jersey:—"A series of earthquake waves passed here at 8.14 this evening: I could not be quite sure of the direction, but I think from south-west to north-east. They continued for forty seconds at least. I was in my room at the top of the house, and so felt the full force. The whole room trembled, windows rattled, and at the same time the room swayed gently. Some one at the time ran along the road shrieking, 'Earthquake! earthquake!' I think that this is the most severe shock that has been felt in Jersey for many years."

At Sark, also, the earthquake was noticed. Major R. D. Gibney, writing to us from that island on May 30, says:—"This evening I felt two distinct shocks of earthquake, the whole lasting about three seconds, the direction being from south to north-west. Time (believed to be Greenwich) 8.25 p.m. The night was clear, but the setting sun and angry clouds hanging on the horizon presaged rough weather. The shock or shocks were sufficiently severe to shake furniture and to rattle crockery on shelves in almost every house in the island. A low rumbling noise, somewhat like distant thunder, accompanied the vibrations."

Three sharp shocks were felt on the same evening at Cherbourg, but the time has not been exactly noted. The cornice of the doorway of the Church of the Holy Trinity was thrown to the ground. The earthquake is said to have been distinctly felt at Havre, Granville, Caen, and Rouen; and it is stated that it was also felt in Paris at certain points on the left bank of the Seine.

Correspondents have written on the subject to NATURE and the daily papers from many different parts of the southern coast of England. A writer at Penzance testifies that three or four shocks were felt there at 8.21, the direction being from west to east. Mr. J. M. Hayward, writing to us from Sidmouth, on May 31, says:—"As I was sitting here alone quietly reading yesterday evening, I felt a very decided shock of earthquake, three distinct vibrations, each of which shook my chair to and fro several times, and made the things on the table—a china plate with a small glass of flowers in it—rattle; the last was strong enough to make me put down my paper, take off my glasses, and wonder whether the room would tip over. I immediately made a note of the time, 8.20, but I cannot answer for my clock being exactly right." At Blandford, Dorset, a vibration, which is said to have occurred at 8.18, lasted about ten seconds; and Mr. G. J. Groves states that the glass and china ornaments in the room in which he was sitting rattled audibly. According to the Rev. L. Lester, a distinct shock was felt at Wareham, Dorset, "about a quarter past 8." "It happened," he says, "while we were in church. There was first of all a very slight shock, which caused the roof to crack, as it does sometimes in a strong gale; but immediately after there was one much more severe, strong enough not only to make the roof-timbers crack in a far greater degree, but also to set the lamps in the chancel swinging. Those of the congregation who happened to be sitting in seats attached to the main piers or pillars of the church felt a distinct movement. The direction of the shocks seemed, by the way in which the noise ran along the roof, to be from north-west to south-east." At Poole the shock was so severe that many persons rushed from their houses in alarm. Colonel L. S. Venner, writing from St. Rode, Bournemouth, says that at 8.20 p.m. a shock of earthquake passed through the house, travelling from south to north. "The features of it were a strong quivering of the floor, with an up and down movement, accompanied by a hollow noise underneath, and the shaking of shutters and crockery. An invalid's bed was a good deal oscillated, and a dog on it was alarmed. The shock was not at all violent. The wind, which was cold and fresh from south, became still just before the shock, and then freshened up again. The sky was clear." Mr. J. Grey, also writing from Bournemouth, fixes the time at 8.18. He and three others were in a ground-floor room facing the sea, when they felt two shocks: it seemed as if the floor was upheaving. The servants at the back part of the house did not notice it. At Portsmouth, Havant, and the surrounding district the shock is said to have been felt about 8.25; and at Havant, where articles were visibly moved, there was "considerable alarm." In many parts of the Isle of Wight the earthquake attracted notice, and at Sandown and Shanklin the residents are said to have been "greatly alarmed." Dr. F. M. Burton, the senior curate of Newport, writes:—"We were in church, attending the evensong of Ascension Day. It was about the middle of the sermon, when a tremor passed through the church, apparently from north to south. The roof groaned and cracked; the reading-desk in which I was seated (a solid old oak structure, of 1636) perceptibly and very unpleasantly moved, and the gas standards shook for some minutes after. Our vicar, the archdeacon of the island, and several members of the choir and congregation observed it. One lady (Mrs. Haigh) had the presence of mind to time the shock, which took place about twenty-three minutes past eight. Several people told me next morning that they felt the shock more or less severely in their houses." Writing from St. Laurence, Ventnor, Mr. W. E. Kilburn says:—"A very distinct shock of earthquake occurred here at 8h. 21m. 30s. p.m. The shock was not sufficient to overthrow anything or to stop the clocks; but the long pendulous drops, 8 inches long, of the glass lustres on the solid marble chimney-piece formed admirable seismometers, vibrating freely for twenty minutes afterwards, and showing the direction apparently from the south-west. The aneroid and barometer indicated 29·73 at the time; they were 29·80 at 9 o'clock in the morning. The temperature in the open air was 53° by Casella thermometer, and the wind south-south-west. The duration of the shock was about three seconds." The Rev. A. Conder says that at Bognor there were two severe shocks at 8.20, with an interval of about three seconds between them:—"An invalid in an adjoining house called for assistance, as some one was under her bed lifting it. I distinctly felt the shocks, which caused the window-frames to rattle." At Littlehampton, at 8.22, cranes were seen to swing suddenly, and an oscillation was felt in different parts of the town. At Arundel, while reading on a sofa, Mr. E. Goldsmith suddenly "felt a peculiar movement, and distinctly saw the sofa vibrate for three or four seconds." "At the same time," he says, "the windows shook, not as they had occasionally done during the evening on account of the wind, but with a quite different and more continuous movement. I called to my daughter, who was at the moment in the dining-room, to know what was the matter; but she had only heard the windows shake in the room where she was (but in the same peculiar manner), and had not felt any movement. My little boy, who was in bed upstairs, felt his bed move; and my two little girls, who were going to bed, were quite frightened, and ran down to know if it was an earthquake. The servant with them felt it too. The time was 8.20." At Brighton a distinct shock was felt, especially in the western part of the town; and at Rudgwick, near Horsham, two persons in a room noticed a movement "which caused a rocking of chairs, cracking of woodwork, and the sound of rumbling, apparently exhausting itself to the westward." Captain H. King, R.N., writes to us from Petersfield:—"On the evening of May 30, at 8.20 p.m., I was leaning upon a spring mattress, when I felt it vibrating in a peculiar manner. We could only account for it by an earthquake; and surely enough the newspaper of next day described one at Guernsey, which appears to have been similar to one which I witnessed in Jersey in April 1853." Mrs. Lane, also writing to us from Petersfield, says that she and her children's governess, while sitting together, "were startled by a most peculiar vibration seeming to shake the house, which quivered perceptibly for some seconds."

Several correspondents of the *Times* testify that they felt the

shock in London. Mr. Ernest Myers writes from 31 Inverness Terrace, W.:—"A slight but unmistakable shock was felt here about 8.20 p.m. There was no rattling of windows or other sound. The vibration seemed to be from side to side." Mr. E. W. Haines, of Alexandra Road, St. John's Wood, says:—"The earthquake was distinctly, though slightly, felt here last evening at 8.30." A member of the firm of Yates, Crighton, and Co., of Cannon Street, E.C., while working in their offices on Thursday evening, distinctly felt four shocks just before 8.30. He says:—"It was the more noticeable as our offices are situated in a huge building, on the third floor, and the sensation was just as if the whole block were rocked by the wind from south to north." "C. W. H." writes from the General Post Office:—"Last evening I was sitting in my room, situated in the south-west corner, top story of the General Post Office, when I felt my chair oscillate with a slight tremulous motion, which lasted perhaps four seconds. Thinking it was a slight shock of earthquake, I stood up, and looking at my watch saw the time was 8.20." A person living at West Kensington reports having felt the shock at 8.15. Mr. F. Yates, writing from Park Street, Southwark, S.E., May 31, says:—"Yesterday evening, between 8.20 and 8.25, while sitting in my library at Surbiton, I distinctly felt two light shocks, which I attributed to earthquake. The shocks were also observed by other members of my family."

Mr. J. Lloyd Bozward writes to us from Henwick, Worcester, that the earthquake was perceptible there. While seated in a room on the second floor of his house at about 8.23 p.m. on Thursday, all being still, he felt five distinct tremors in rapid succession, the third being the most notable. "On making immediate inquiries," he says, "I learned that the tremors had not been felt on the other floors, but my son, who happened to be in the basement on the occasion, says that at the time referred to by me he noticed that the flame of a lamp burning on the table suddenly shot up above the top of the glass chimney."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Annual Report of the Museums and Lecture Rooms Syndicate, just issued, contains much interesting information about the progress of natural science studies and collections.

Prof. Babington announces that the late Prof. Churchill Babington's extensive herbarium has been presented to the Botanical Museum by his widow, including the typical specimens of lichens described by him. The type collection has been enlarged, and demonstrations in organography and histology are regularly given. Mr. Potter has just returned from Ceylon with a fine collection. A series of germinating seedlings (prepared by Mr. Barber), specimens showing the injuries caused to plants by insects (by Mr. Shipley), and Kny's diagrams, given by Mr. Thiselton Dyer, are among the valuable acquisitions.

Mr. J. W. Clark, Superintendent of the Museum of Comparative Anatomy, reports the gift of a beautiful collection of spiders, with accompanying drawings, by Mr. Warburton; the mounting of the skeleton of *Rhytina gigas;* the deposit of a valuable collection of skulls and bones of Bovidæ and Cervidæ, by Mrs. Stewart, widow of Surgeon-General L. C. Stewart; Surgeon-General Day has given 357 birdskins from India and Burmah; and Messrs. Cordeaux have given over 100 valuable Indian specimens.

Two parts of the "Morphological Studies" have been issued by Mr. Sedgwick since the last Report. The Elementary Biology Class numbered 167 in the Easter term of 1888, and 139 in the Lent term of 1889. The Morphology Class varied from 77 to 42; with a smaller advanced class.

Prof. Macalister reports the addition of 131 Egyptian skulls, 25 skulls from the Saxon burial-place at Hauxton, and many from that behind St. John's College. The Rev. J. Sanborn, of Lockport, N.Y., has given valuable skulls from a burying-place of the Seneca Indians.

Prof. Roy describes the careful and systematic arrangement he has adopted in his Pathological Laboratory (late the Chemical Laboratory).

Prof. Hughes once more deplores the long postponement of the new Geological Museum. It certainly is not just to allow the donors of the funds to die out and never see the erection of the Museum towards which they contributed such large sums. Important additions have been made to the Cambrian and Silurian fossils by Mr. Marr, and many of them have been described and figured by him. Thirty-four figured types from the Inferior Oolite of Dorsetshire have been presented by the Rev. G. F. Whidborne. About 130 slides have been added to the cabinet of microscopical preparations of rocks. Much progress has been made in palæobotany, and two courses of lectures have been given by the lecturer, Mr. Seward. Mr. Strickland's collection of fossils, numbering 7000 specimens, has been presented by the late Mrs. Strickland.

The new Chemical Laboratory proves to be very satisfactory in working.

The demonstrations in the Cavendish Laboratory were attended by 136 students last Michaelmas term and 144 in the Lent term. Twelve persons have been doing original work in the Laboratory during the year. Some important new apparatus has been acquired.

SCIENTIFIC SERIALS.

Mémoires de la Société d'Anthropologie de Paris, série ii., tome iv., fasc. 1 (Paris, 1889).—Pre-Columbian ethnography of Venezuela, by Dr. Marcano. The author prefaces his special ethnographical remarks with a short geographical notice of the Venezuelian territory, entering more particularly into the physiographical character of the fertile valleys of Aragua and Caracas. The special feature of the landscape in these picturesque regions is the range of low hills locally characterized as "Cerritos," which extend over a large area near the beautiful lake of Valencia, first known to the Spaniards as Lake Tacarigua, and which were regarded by the native Indians as natural features of the soil. It has been discovered, however, by recent explorers, that they are artificial elevations, raised in past ages by some aboriginal Indian race long extinct, whose very name is unknown to the present inhabitants of the district, although the shores and bottom of the lake testify, through their vast accumulations of bones and other *débris*, that the country must have been densely populated at some remote prehistoric period. Dr. Marcano, who devoted several years to the exploration of the Cerritos, near Lake Valencia, has succeeded in laying bare the interiors of twenty of these mounds, which prove to be sepulchral caves filled with bone and other detritus. All present a uniform plan of arrangement, and consist of a central circular walled-in space, containing an enormous mass of whole and fractured bones, and marine and fresh-water shells, with fragments of stone, bone, and wood implements, and sherds of pottery, most of which bear traces of the action of fire. The human remains were deposited in round earthen jars or urns, each of which contained only the separate bones of one body, the skull resting at the base of the vessel, while the sacrum, with the long and the small bones, was laid above it so as to fit into all the available space. The appearance of these bones indicates that the flesh had been detached from the dead body before its interment, but their brittle condition rendered a minute examination impossible in some cases, although Dr. Marcano was able to recover forty crania which admitted of sufficiently exact investigation to warrant the conclusion that they represent two distinct types of brachycephalism. About half of these crania showed signs of deformity, due to artificial pressure over the frontal bones. The most remarkable characteristic was their prognathism, which exceeded that of any skull previously examined by him, although his observations were based on the examination of more than 2000 crania, of which some belonged to New Caledonians, who have hitherto ranked as belonging to the most prognathic race extant. The implements found in the Cerritos caves are nearly identical with those associated with the Neolithic age in Europe, while the animal remains are composed of types belonging to the local terrestrial and aqueous faunas, including the broken skull of a cebus; while so enormous a mass of the bones of a caïman (*Crocodilus bava*, which is peculiar to the Lake of Valencia and its affluents) was found, that it is evident the flesh of this animal must have served as food. A number of detailed craniological tables, and numerous illustrations of the crania and of the curious figurines and idols, the urns, tools, ornaments, and other objects interred with the human bones, add greatly to the value of Dr. Marcano's exhaustive memoir.—The superstitions prevalent in Wales, by M. Maricourt. In this article the author has drawn his materials so indiscriminately from

casual travelling companions, and from writers of the most various degrees of authority, that his statements can lay no claim to the serious attention of students of folk-lore, and present no interest for the English reader.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, May 25.—Prof. Reinold, President, in the chair.—The following papers were read:—On a relation existing between the density and refraction of gaseous elements and some of their compounds, by Rev. T. Pelham Dale. In a previous communication the author pointed out a relation between the specific refractive energies of sulphur and selenium, and the present paper deals with similar relations in gaseous bodies. On calculating out the values of $\frac{\mu - 1}{d}$ for the elements H, O, N, Cl, S, P, it was noticed that the logarithms were nearly integral multiples of half the logarithm for H, those for N, Cl, and P being double, and S and O three times that number. The value of $\mu - 1$ for different elements is compared with the $\mu - 1$ for H, the resulting numbers being, for oxygen 2, mercury 7, arsenic 8, and sulphur 12 nearly. Similar calculations are made for the compounds N_2O, NO, CO, SO_2, Cy, NH_3, HCl, H_2S, CH_4, and C_2H_4, but as the data obtainable are very rough, the numbers are not so closely integral. The author hopes that better data will be furnished by persons having greater facilities than himself for experimental research. Prof. Rücker thought the results obtained pointed towards some relation between the volumes of the molecules of different elements, and at the close of the meeting announced that on working out the relation he found the relative volumes to be a series of numbers in geometrical progression.—On a water-spray influence machine, by Mr. George Fuller. The apparatus is made up of four similar sections, each consisting of a nozzle, a metal ring, and a metal dish or receiver, arranged about a vertical axis. Pressure-water issues from perforations 1/100 inch in diameter in the nozzles, and passes through the rings into the insulated receiver below. The rings are placed at such a distance below the nozzles as to be about the point where the streams break into spray, and the receivers empty themselves automatically. Calling the consecutive sections 1, 2, 3, 4, respectively, the rings of 1 and 3 are connected to the receiver of 4, and those of 2 and 4 to the receiver of 1. The discharge-points are connected with the receivers 2 and 3, and a rapid succession of sparks passes when the water is turned on. Prof. S. P. Thompson inquired whether the length of the spark was limited by leakage along the glass rods or by the spray passing between the receivers, and in reply Mr. Fuller said he thought the former leakage the most important.—Notes on polarized light: (*a*) on the transition tints of various orders; (*b*) lecture illustrations of the rotation of circularly-polarized light; (*c*) on the rotation of circularly-polarized and non-polarized light, by Prof. S. P. Thompson. The first note described an inquiry as to what thickness of quartz gives the best "sensitive tint" for polarimetric work. Biot gave the name to the tint produced by a quartz $3\frac{3}{4}$ millimetres thick in a bright field, whereas in most modern polarimeters the name is given to that produced by $7\frac{1}{2}$ millimetres of quartz in a dark field. The transition-tints of various orders were exhibited on a diagram of Newton's scale of colours, and by a wedge of selenite. Experiments were made on quartzes of $3\frac{3}{4}$ and $11\frac{1}{4}$ mm., giving tints of the first and second order respectively in the bright field, and with a $7\frac{1}{2}$ mm. quartz in a dark field. The $3\frac{3}{4}$ was more sensitive than the $11\frac{1}{4}$ to small rotations, but the $7\frac{1}{2}$ mm. seemed the best of the three. Prismatic analysis of the light transmitted by each led to the same conclusion—a new square-ended direct-vision prism built up of a glass prism (angle 140°) immersed in cinnamic ether being used for that purpose. The author pointed out that the "sensitive tints" of German opticians are decidedly redder than Biot's, and those generally used in England. In the first apparatus devised under (*b*), the ray of light is represented by a stretched cord thrown into promiscuous vibration by a tuning-fork, and the polarizer and analyzer are each represented by two plates of glass mounted parallel to each other about a millimetre apart, between which the cord passes. Between the polarizer and analyzer the vibrations are in one plane, and they are transmitted or cut off by the analyzer according as its plates are parallel or perpendicular to those of the polarizer. By blowing across one side of the spindle-shaped vibrating segment between the crossed plates, the plane of vibration is slightly rotated, and part is transmitted through the analyzer. Other experiments illustrating rotation of the plane of polarization were shown or described, the most conclusive being a bar of heavy glass placed along with a fish-eye lens between crossed Nicols. On starting a current in a helix surrounding the glass, the black cross formed by the fish-lens is seen to turn round as the current grows. Another piece of apparatus to illustrate Fresnel's view of the circularly polarized waves in quartz consists of two equal coplanar disks rotating in opposite directions, and carrying pins on which the extremities of a double pantograph arrangement are pivoted. The middle point of the link-work describes a line perpendicular or inclined to the line of centres of the disks according as the phases of the pins are the same or different. (*c*) In speaking of rotary polarization it is customary to say that the plane of polarization is rotated, but the author thinks it is equally correct to say that the light itself is rotated. Prof. Stefan's and Prof. Abbe's experiments bearing on the subject were described, and to demonstrate that ordinary light may be rotated, a biquartz was placed between a Fresnel biprism and the screen on which the interference fringes were formed. By using quartzes of thickness sufficient to rotate each beam 45°, the interference fringes are caused to disappear, and on inserting a bar of heavy glass in each of the pencils, and magnetizing one of them, the fringes reappear. Mr. Glazebrook thought the reason why $3\frac{3}{4}$ mm. quartzes were more sensitive than $11\frac{1}{4}$ might be seen by considering the sector-shaped spectrum in which the rays are spread out by the quartz, for with the thick piece the angle of the sector will be three times that with the thin one, and hence, in the latter case, a greater change of colour is produced by a given small rotation. Mr. Ward strongly condemned the use of biquartzes for rotation measurements, for he found it impossible to get them cut with such accuracy as to give a uniform tint; and if the light be slightly elliptically polarized, considerable error may be introduced. Speaking of magnetic rotation, he thought Fresnel's explanation unsatisfactory, and considered it probable that the rotary character of the magnetic field increases the period of one and decreases that of the other circular wave, their velocities remaining the same. As regards quartz, he believes the rotary action due to the light itself (probably an effect of the longitudinal wave), and not to any peculiar crystalline structure of the quartz, for liquids exhibit similar phenomena. Dr. Thompson, in reply, said Mr. Glazebrook's explanation of the difference in sensibility of the quartzes of various thicknesses was not quite satisfactory, for the reasoning would lead one to expect the $3\frac{3}{4}$ millimetres to be most sensitive, whereas experiment showed that the $7\frac{1}{2}$ millimetres was best. He quite agreed with Mr. Ward about the defects of the biquartz, and thought the shadow method preferable in many cases. On the other hand, he was disposed to believe that the rotary power of quartz was a result of its crystalline structure, for fused quartz possessed no such property. As regards liquids, Dr. Thompson thought the rotation due to some kind of skew symmetry possessed by the molecules, the average effect of which is observed.—On the molecular weight of caoutchouc and other colloid bodies, by Dr. J. H. Gladstone, F.R.S., and Mr. W. Hibbert. This paper gives the results of determinations made by Raoult's method, the reliability of which was first tested by preliminary experiments on substances of known molecular weights, and found to be fairly satisfactory. The experiments on caoutchouc give a very high value (above 6000), thus confirming the author's previous impression that it was a colloid. Similar experiments were made on gum-arabic, caramel, albumen, and ferric and aluminic hydrates, all of which were found to have high molecular weights. All the experiments confirm the belief that the molecule of a colloidal substance is an aggregate of a very great number of atoms.

EDINBURGH.

Royal Society, May 20.—Sheriff Forbes Irvine, Vice-President, in the chair.—A paper, by Prof. Letts and Mr. R. F. Blake, on the identity of Hofmann's "dibenzyl phosphine" with oxide of tribenzyl-phosphine, and on some other points connected with the phosphorized derivatives of benzyl, was read.—Sir W. Turner communicated a paper by Dr. D. Hepburn, on the development of diarthrodial joints in birds and mammals.—Dr. G. Sims Woodhead communicated observations by Mr. D. McAlpine on the progressive movement of detached ciliated portions of frogs and tortoises, and also observations on the progression, pulsation,

and quivering of excised hearts of fish, frogs, reptiles, birds, and mammals.—Dr. John Murray read a paper by Mr. W. S. Anderson, on the solubility of carbonate of lime in fresh and sea water.

PARIS.

Academy of Sciences, May 27.—M. Des Cloizeaux, President, in the chair.—On the radicular nature of the stolons of *Nephrolepis*, by M. A. Trécul. In reply to M. Van Tieghem, the author argues that the so-called stolons of this fossil plant were not shoots or runners, but had the constitution of true roots; hence he was fully justified in describing them as "radicular stolons."—On the representation of the continuous fractions expressing the two roots of a quadratic equation, by Prof. Sylvester. In continuation of his previous paper (May 20), the author shows how the twin formulas there worked out for the two roots x and x' may be considerably simplified and generalized.—On the Calamariæ, *Arthropitus* and *Calamodendron*, by M. Grand'Eury. In this paper the author sums up his reasons for believing that these Carboniferous plants were highly organized Cryptogams. Their representatives or descendants in the Secondary formations were of smaller size, less varied, and more nearly related to *Equisetum*, the last degenerate survivor of the family.—Exact determination of the quantity of water contained in the blood, by MM. Gréhant and Quinquaud. Experiments made on the dog and rabbit show generally that the quantity of water is less in the venous than in the arterial blood. For the dog, the respective proportions were found to be: water, 77·09; dry residuum, 22·91; and 78·01 and 21·99 per cent.—Quantitative analysis of the urea contained in the blood and in the muscles, by the same physiologists. From these experiments, made with the rabbit and the skate, it would appear that the muscles of this fish contain fifty times more urea than those of mammals, and that the urea is formed in the muscles, in which it is present in larger quantity than in the blood.—Distribution in latitude of the solar phenomena during the year 1888, and solar observations for the first quarter of 1889, by M. Tacchini. The results of the observations here tabulated show that in 1888 the solar phenomena were much more frequent in the southern than in the northern hemisphere. The maximum zone for the spots, faculæ, and metallic eruptions lay between 0° and −10°, as in 1886 and 1887. But the maximum for protuberances does not correspond with that for the other phenomena, as it lay in higher latitudes (+30 to +40 and −40 to −50). The observations for the first three months of 1889 show a perceptible diminution of the spots and faculæ as compared with the last quarter of 1888, while the protuberances were somewhat more frequent in the former than in the latter period.—On the expansion of the metals at high temperatures, by M. H. Le Chatelier. In continuation of his previous communication the author here tabulates the results obtained for iron, steel, copper, aluminium, silver, nickel, platinum, and sundry alloys, concluding generally that for all metals the coefficient of expansion increases with the temperature. The law of increase is generally regular except for certain alloys of silver and for all varieties of iron.—Researches on the phenomenon of magnetic rotatory polarization in Iceland spar, by M. Chauvin. In a previous note (April 27, 1886) the author showed, against the opinion of Wertheim, that this substance possesses magnetic rotatory power not only in the direction of the axis but also in the neighbouring directions. His further researches here summarized generally confirm this conclusion, the phenomena observed being identical with those presented by natural quartz.—On the electric conductivity of saline solutions: reciprocal displacements of the acids, by M. P. Chroutschoff. A tabulation of the chief results already obtained of the reactions between salts in solution and acids other than those entering into the composition of the salt under examination.—Researches on the electric resistance of bismuth, by M. Edmond van Aubel. The author here studies the influence of temperature on the electric resistance of bars of bismuth, and examines this metal under two molecular states: melted and slowly cooled; melted and very rapidly cooled or tempered.—On the heat of combustion of some organic bodies, by M. J. Ossipoff. Continuing his determinations of the heat of combustion of organic bodies, the author here deals with racemic acid and its anhydride, and with methyl racemate and tartrate.—On some metallic sulphides, by MM. Armand Gautier and L. Hallopeau. Continuing their last memoir (April 15), the authors here describe the action of carbon disulphide on nickel, chromium, and lead.—Papers were contributed by M. F. Parmentier, on the presence of sulphate of soda in the atmosphere; by M. A. Haller, on a general method of synthesis for the β-acetonic acids of the aromatic series; and by M. E. Sorel, on the rectification of alcohol.—The President announced the death of M. Halphen at Versailles; and M. Hermite paid an eloquent tribute to the memory of the illustrious geometrician at his obsequies on May 23.—The death was also announced of the distinguished physicist and electrician, M. Gaston Planté.

VIENNA.

Imperial Academy of Sciences, March 21.—The following papers were read:—On Van Deen's blood-test and Vitali's pus-test, by E. Bruecke.—On some problems of the theory of the conduction of heat, by T. Stefan.—On the alterations of the pigment in the insect's eye, caused by the influence of light and its physiological meaning, by S. Exner.—New observations on the change of combinations in phenols (third communication), by T. Herzig and S. Zeisel.—On ortho-dicarbonic acids of pyridine, by G. Goldschmidt and H. Strache.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Petrographical Tables: H. Rosenbusch, translated by F. H. Hatch (Sonnenschein).—An Elementary Treatise on Mechanics: I. Warren (Longmans).—The Metallurgy of Silver: M. Eissler (Lockwood).—A Treatise on Geometrical Conics: A. Cockshott and F. B. Walters (Macmillan).—An Elementary Text-book of Chemistry: W. G. Mixter, 2nd edition (Macmillan).—A Practical Guide to the Climates and Weather of India, Ceylon and Burmah, &c.: H. F. Blanford (Macmillan).—Papers on Alternating Currents of Electricity: T. H. Blakesley, 2nd edition (Whittaker).—A Graduated Course of Natural Science, Part 1, 1st Year's Course: B. Lœwy (Macmillan).—International Annual of Anthony's Photographic Bulletin, vol. ii., 1889–90 (Illiffe).—British Dogs, No. 31: H. Dalziel (U. Gill).—La Période Glaciaire: A Falsan (Paris, Alcan).—Indian Meteorological Memoirs, vol. iii. Parts 3 and 4, and vol iv. Part 5 (Calcutta).—Report on the Meteorology of India in 1887 (Calcutta).—An Elementary Treatise on Dynamics: B. Williamson and F. A. Tarleton, 2nd edition (Longmans).—Questions on Stewart's Lessons in Elementary Physics: T. H. Core (Macmillan).—The Middle Lias of Northamptonshire: B. Thompson: (Simpkin).—Essays upon Heredity and Kindred Biological Problems: A. Weismann; authorized Translation by Poulton, Schönland, and Shipley (Clarendon Press).—Himmel und Erde, Heft 9 (Berlin, Paetel).—Aus dem Archiv der Deutschen Seewarte, viii. Jahrgang, 1885 (Hamburg).—Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen, 1er Semestre, 1887 (Habana).—Veröffentlichungen aus dem Königlichen Museum für Völkerkunde, i. Band, 1 Heft (Berlin, Spemann).

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